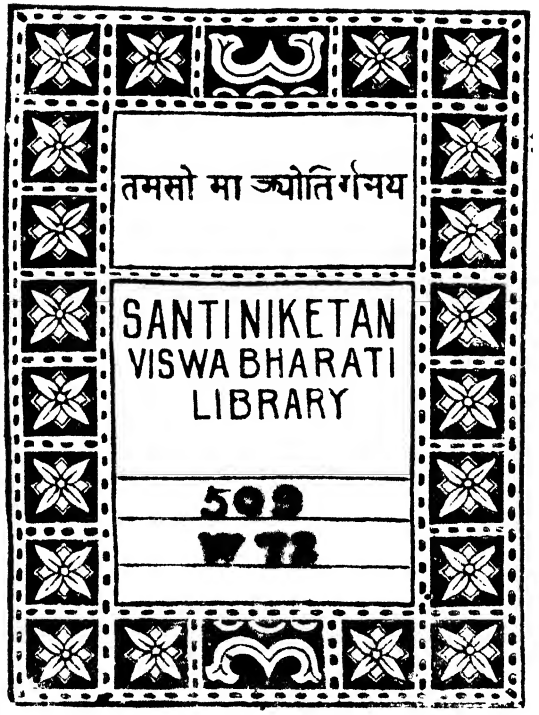
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TO
THE REV. M. MAHER, S.J., M.A., D.LIT.,
IN GRATEFUL REMEMBRANCE OF MUCH
KIND HELP ON MANY OCCASIONS
October, 1915.

THE Author desires to express his thanks to the Editors of the *Dublin Review*, *Catholic World* and *Studies* for permission to reprint articles which have appeared in those Journals.

CONTENTS

A CENTURY OF SCIENTIFIC THOUGHT

A Century of Scientific Thought & Other Essays

By
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I: A CENTURY OF SCIENTIFIC THOUGHT

THERE is a lovely mountain tarn, one of the many in that region of natural beauties known as Kerry, to which my thoughts have often turned whilst thinking over the subject of this article. High up, embosomed in the mountains, for the most part of the year it is even unfishable, because its sheltered position leaves its surface too glassy for the trout to be deceived by the fly. When, however, the wind blows from the right quarter, what a change in the lake! Its mirror-like surface is torn with waves, miniature water-spouts and spindrift rage and tear over its face. It is hardly recognizable as the peaceful thing it was, and will be again. For when the wind dies down, once more the pool returns to its peaceful state; once more puts on its glassy surface. Yet, no doubt, profound changes have taken place in the disposition of its waters: they are not as they were. The depths of the pool have been stirred, all cannot be as it was before the storm raged. The pool is the same, though a profound reconstruction of its constituent parts may have taken place. And any dweller by its shores could tell the casual visitor that such a storm, such a reconstruction, such a subsequent calm, was no unique experience, but a thing which had happened before, and must be expected to happen again. The analogy, like all analogies, breaks down if pushed very far beyond

its confines, but in a general way it seems to me to bear a resemblance to the relations between religion and science. I speak of religion generally, and specially of religion as typified by the Catholic Church. And, though the period with which I am concerned is the last hundred years, similar cycles of events have taken place in the past; can we doubt that their like will take place in the future?

Let us turn our attention to the *Argument from Design*, commonly so-called. My intention is to consider its position at the beginning of the last century, and the effect upon it of the great controversies of the middle of that century which men think of as the Darwinian controversy. Finally, I desire to consider how that argument stands to-day, after the storm and fury of that controversy has abated.

It would be a very great error to suppose that the pre-Darwinian era was one barren of scientific discovery. Quite the contrary was the case. At the opening of the last century, or during the last few years of that which preceded it, the ancient caloric theory of heat was upset by Rumford and Davy, who showed that heat was a mode of motion affecting the molecules of the heated substance. The ancient corpuscular theory of light was also upset by Young, who showed that light was due to a wave motion in the ether, then first described as a new medium. The same authority introduced the concept of energy to the scientific world. Dalton brought forward the atomic theory; Wollaston detected the dark lines in the solar

spectrum; Volta discovered the electric pile. It is true that these epoch-making discoveries were on the physical side of science, rather than on the biological. It is also true that it is from the latter rather than from the former, that we are accustomed in these days to look for conflicts between religion and science.

We cannot, however, forget that the one serious conflict with science in which a mistake was made by the then rulers of the Church, was on the physical side, for the dispute with Galileo raged around the geocentric and heliocentric theories of astronomy. With the merits of this dispute I cannot now afford the time to deal, though there is much that could be said about it. Here I will only note in passing that, as Newman remarked, it is the one and only definite case which can be brought up, and is invariably brought up, as an example when the Church is accused of being the enemy of science. Huxley hated our religion and—very foolishly and very ignorantly too—rejoiced that evolution "in addition to its truth has the great merit of being in a position of irreconcilable antagonism to that vigorous enemy of the highest life of mankind—the Catholic Church!"* Yet this same Huxley, having studied the case of Galileo, took up a position which I must confess I myself should find it difficult to defend, that "the Pope and the Cardinals had rather the best of it."† And according to Sir David Brewster, Leibnitz declared that the theory of gravitation was opposed

* *Darwiniana*, p. 147.
† *Huxley's Life and Letters*, vol. ii, p. 113.

to natural religion. This view was never supported, as far as I am aware, by any authority, nor do I see how it ever arose. I mention it merely to show what curious positions can be assumed by men of the first order of intellect, when their minds have been enthralled by, and wedded to, some particular theory. At any rate this may be said of the pre-Darwinian portion of the era with which I am concerned, that, from the biological side of science, there arose no serious, even considerable theory which was taken to be opposed to religious doctrine.

In the early days of the last century, in 1802 to be precise, there appeared a work which may fairly be looked upon as the high watermark of pre-Darwinian apologetics in England. I allude to that very remarkable work on natural theology, commonly known as Paley's *Evidences*. Fallen now, it would seem, and for reasons which will shortly appear, into quite undeserved neglect, Paley's book was well-known to the great biologists of the Darwinian upheaval, and highly appreciated by them. Darwin himself said, "I do not think I hardly ever admired a book more than Paley's *Natural Theology*. I could almost formerly have said it by heart."* Huxley knew and grasped the real teaching of Paley a good deal better than many of his opponents, who would no doubt have thought of themselves as ranged under the banner of Paley. And Huxley very acutely calls attention to the remarkable argument in the twenty-third chapter of the *Evidences*. Here Paley argues that "there

* *Darwin's Life and Letters*, vol. ii, p. 219.

may be many second causes, and many courses of second causes, one behind another, between what we observe of nature and the Deity: but there must be intelligence somewhere; there must be more in nature than what we see; and, amongst the things unseen, there must be an intelligent, designing Author." Then he proceeds to point out how things around us are produced by the adding of particles to one another, so as to become collectively organized bodies by means of motions which we cannot explain. "There may be," he continues, "particular intelligent beings guiding these motions in each case; or they may be the result of trains of mechanical dispositions, fixed beforehand by an intelligent appointment, and kept in action by a power at the centre. But in either case there must be intelligence." In reference to which Huxley adds, "that is to say, he proleptically accepted the modern doctrine of evolution, and his successors might do well to follow their leader, or at any rate to attend to his weighty reasonings, before rushing into an antagonism which has no reasonable foundation."*

Paley's line of argument is, or ought to be, well-known to all educated persons, and need only be outlined here in the briefest possible manner. He commences by supposing himself to be walking upon a heath. He strikes his foot against a stone: it is clear that it may have been there for ever; at any rate, to the casual observer, it presents no special problems for solution. But a little further on he comes upon a watch, and that can hardly be

* *Darwin's Life and Letters*, vol. ii, p. 202.

explained in the same manner, namely, that it may have been always there. He develops his argument by explaining the structure of the watch, and by claiming the inevitable inference that the watch must have had a maker, "an artificer or artificers, who formed it for the purpose which we find it actually to answer: who comprehended its construction, and designed its use." Nor in his opinion are the arguments weakened by the facts that: First we may never have seen a watch made, or known an artist capable of making one; second, that the watch sometimes, even frequently, went wrong; third, that there were parts in it which we did not understand. Further, he argues that the finder of the watch could not be expected to be satisfied by any of the following arguments: First, that it was one out of many combinations of matter, and might have been thus or otherwise arranged; second, "that there was a principle of order which had disposed the parts of the watch into their present form and situation"; third, that the mechanism was no proof of contrivance, only a motive to induce the mind to think so; nor again by the argument that, fourth, the watch was no more than the result of the laws of metallic nature. Finally, he is not to be put off from his belief by being told that he knew nothing of the matter. Paley then applies his argument to different parts of the human body, and subsequently to various contrivances throughout the animal kingdom.

For example, he considers and describes the human eye; a marvel of contrivance. He points out

that it has, like the telescope, lenses and means for focussing, and other like things. If the devices we meet with in the telescope are designed, as unquestionably they are designed, to aid our vision, can we doubt that the devices we find in the eye are also designed for the very purposes of vision itself? And in a similar manner he deals with a number of other contrivances in man and in the lower animals, which exhibit, according to his theory, undoubted evidences of design.* Such was the thesis with whose development Paley's work is concerned, and it embodies the *Argument from Design* as it was stated at the beginning of the nineteenth century, and, indeed, as it continued to be stated up to the Darwinian days. Into this comparatively peaceful pool rushed the whirlwind of Darwin with the subsidiary and adjuvant blast of Huxley—a nipping and an eager air from a keener and adjacent quarter. Let us try to take stock of the events of that period. Darwin's great book is commonly and most misleadingly known to the majority of mankind as *The Origin of Species*, whereas its true title was *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*. The distinction which I have drawn between the popular and the actual titles is not a mere piece of pedantry: behind it lies a historical fact of the utmost importance.

Darwin was by no manner of means the first person to propose the theory now commonly but incorrectly known as "Darwinism," but which is

* The outline is that of the first chapters of the work in question.

more accurately described as "Transformism," *that is, the theory of the derivative character of living things. What Darwin did was to put forward* a means to explain this derivation, namely, Natural Selection, to which he added some subsidiary factors such as Sexual Selection. I have no time to deal in any adequate manner with the pre-Darwinian transformists, but this may be said, that, apart from pagan writers who foreshadowed it at least, it has in its essence been a subject of discussion amongst the great Catholic writers since the time of St. Augustine of Hippo. I am not enough of a theologian to decide, nor, since theologians differ on the point, am I sufficiently foolish to attempt to decide, how far St. Augustine was or was not what would be called an evolutionist to-day. To me, at least, it seems as if the language of Peter Lombard and of St. Thomas Aquinas in commenting on St. Augustine, makes it clear that the teaching of the greatest and most influential Doctor in the history of the Church is quite consonant with any reasonable theory of evolution—nay that it is broad and comprehensive enough to provide not only for whatever limited degree of evolution is yet fairly established, but even for *anything that has even a remote probability of being proven in the future.* Nor am I deterred from coming to that conclusion by the very obvious criticism that the Saint did not state the doctrine with the clearness with which it is now laid down, a thing which no reasonable person would expect him to have done.

It seems to me that he stated it "proleptically,"

as Huxley said of Paley. But let that pass; what clearly emerges from the controversy is that a theory in its essence quite undistinguishable from what we call evolution, has been under discussion ever since the time of St. Augustine amongst Catholic theologians. And to turn to other people; of course Erasmus Darwin, Lamarck, and Chambers in his *Vestiges of Creation* put forward transformist views, though without exciting any very great interest, certainly without provoking any very active controversy.

How was it then that Darwin's work aroused the storm which it did? This is a point which we may well consider, since there are important lessons to be learned from it. In the first place the book arrived at what the trite phrase calls the "psychological moment." Scientific opinion, more or less prepared by previous writings, was like a salt in strong solution. Darwin's book was the added crystal which caused the solidification of the whole. But while this is so, the main cause of the success of the book was its own excellence; the careful collection of facts; the patiently elaborated argument; its modest restrained presentation; and, above all, the production for the first time of a theory which purported to explain, and did in large measure explain, how that transformism, in which many had believed, perhaps in a somewhat indefinite manner, could be conceived to have come about. In addition to these reasons there is a third which must never be left out of consideration. The book became a party cry. This most unfortunate circumstance was, I at once and

fully admit, in a very large degree due to the ignorance and the unreasonableness of its opponents, the religious party, chiefly, if not entirely, I am glad to say, drawn from outside the ranks of our own body. It has always seemed to me that in cases of this kind the advice of Gamaliel is golden, and that one should wait and see what the decision of scientific men, at any rate, is going to be before debating the question even on philosophical lines, "for if this council or work be of men, it will be dissolved." Such however, was not the action of some of the defenders of religion; which might indeed have prayed to have been delivered from its friends on this as on some other occasions. It is impossible here to enter into the history of those times. Those who do so will find in the speech of the then Bishop of Oxford, the well-known Samuel Wilberforce, a most admirable example of how not to do it. In his, as in many of the speeches and writings of that day, will be found a neglect of the plain common-sense rule that it is well to understand your opponent's case, and the facts upon which it rests, before entering into argument with him. I wish this simple lesson could be imprinted on the minds of those who essay criticism of *Catholic doctrine and Catholic philosophy. But* this is too much to expect.

Now one result of the rapid acceptance of Darwin's views, the full bearing of which could not accurately be appreciated at the moment, was the apparent destruction of the *Argument from Design.* Darwin asserts this himself, and asserts it with regret, as might be expected from his

admiration of Paley's book. "*The old argument* from design in nature as given by Paley," he writes,* "which formerly seemed to me so conclusive, fails now that the law of Natural Selection has been discovered. We can no longer argue that, for instance, the beautiful hinge of a bivalve shell must have been made by an intelligent being, like the hinge of a door by man. There seems to be no more design in the variability of organic beings, and in the action of Natural Selection, than in the course which the wind blows." And this he says, though a few lines further down he alludes to "the endless beautiful adaptations which we everywhere meet with."

When a new fact of the first importance or a new theory of really wide-reaching importance, is thrown into the scientific arena, it not only creates a vast turmoil there, like the northern blasts on the mountain tarn, but it also necessitates a re-orientation of all kinds of matters, not at first sight connected directly with the fact or theory itself. In our own days the discovery of radium and of radio-activity, has completely altered the attitude of science towards all sorts of subjects, even for example, the age and possible destiny of the sun and the earth. We used to be told that the earth was gradually cooling, and would become an extinct "has-been" like the moon. Yet now there is a school of scientific men which declares that, so far from this being the case, the earth is actually growing hotter and hotter in its interior, and that, if this process goes on, as

* *Life and Letters*, vol. i, p. 309.

go on it apparently must, "at some time or another the world must explode, when the increasing temperature and pressure within overpowers the strength of the crust. According to the same authority, there is no assurance that such a consummation does not await the future, nor evidence that such has not more than once been an event of the past."* As this termination is not expected for something like one hundred million years, we need have no personal alarm, nor need we tremble on this account for the future of our children. I mention the matter to show the far-reaching consequence of a really first-rate discovery or theory.

Such was Darwin's theory, and it is not to be wondered at that the *Argument from Design* required reconsideration and reconstruction in view of his teachings. What the result was will shortly be considered. Meantime it will be sufficient to note one result of the appearance of the book, and more especially of the unfortunate manner in which it was met by those who would have been well-advised to have exercised more caution and discretion in their attitude towards it. This serious result was the loss of faith in revelation on the part of a large number of perfectly honest and even reluctant persons. Perfectly honest: no one can doubt that who reads the remarkable letter which Huxley, after the death of his son, wrote to Kingsley, in which he says that whatever the consequences, he will not try to make himself believe that which in his heart he feels to be a lie. Reluctant

* Soddy, *Matter and Energy*, Home University Library, p. 237.

and regretful : as witness the bitter cry of Romanes when he had lost that belief in Christianity which he regained in his latter days.

"Forasmuch as I am far from being able to agree with those who affirm that the twilight doctrine of the 'new faith' is a desirable substitute for the waning splendor of 'the old,' I am not ashamed to confess that, with this virtual negation of God, the universe to me has lost its soul of loveliness ; and although from henceforth the precept 'to work while it is day' will doubtless but gain an intensified force from the terribly intensified meaning of the words that 'the night cometh when no man can work,' yet when at times I think, as think at times I must, of the appalling contrast between the hallowed glory of that creed which once was mine, and the lonely mystery of existence as now I find it—at such times I shall ever feel it impossible to avoid the sharpest pang of which my nature is susceptible."*

Mr. Chesterton, in one of his delightful flashes of thought, reminds us that "the hardest thing to remember about our own time, of course, is simply that it is a time ; we all instinctively think of it as the Day of Judgment."† Those of the mid-Victorian era had no doubts in their minds that religion and all it entails had come to judgment, and been dismissed with costs. It is perhaps not wonderful that the new wine of scientific discovery, the marvellous outpouring of researches

* *A Candid Examination of Theism*, p. 114.
† *Charles Dickens*, p. 288.

of all kinds in the biological world which followed upon the publication of Darwin's theory, should have got a little into men's heads. Every great *fact, and every potent theory, has this wonderful* thing about it, that it engenders discovery, and Darwin's theory, even as a working hypothesis, has been the direct cause of an extraordinary advance in knowledge during the past fifty years. And as it was ignorantly assumed by some, though not by Darwin nor by Huxley, to have dispensed with any need for a God, that idea was temporarily at any rate overshadowed in, or obliterated from, the minds of men.

There are, perhaps I should say there were, excellent people who really believe that if the Sacred Scriptures in his own tongue were placed in the hands of a heathen who could read, he must *ipso facto* become a Christian. In quite the same way the tendency of the mid-Victorian age was to suppose, that a careful perusal of Darwin's works was enough to shatter the faith of the stoutest. There is a somewhat remarkable novel by a very remarkable, if under-estimated writer, Samuel Butler, called *The Way of All Flesh*. Butler was a real student of the Darwinian controversy, and contributed some pungent writings to it. And no one who was familiar with the mid-Victorian parsonage will dispute the accuracy of many of his pictures of that household. For the rest the figures are somewhat wooden, and in many respects unconvincing. The real point of interest is the faithful representation of the *ethos* of the period, the cocksure attitude which believed that

any rational man who looked into these things could have but one opinion about them, and that was that we neither could, nor need endeavour, *to know anything about God, our souls, a future life*, or other such vain speculations of theologians and philosophers. This attitude is the very atmosphere in which the book was created, and which it exhales to those who read it to-day. And yet it was but a time and not the Day of Judgment. "Few people," says Mr. Chesterton in continuation of the text already cited, "few people for instance, realize that a time may easily come when we shall see the great outburst of science in the nineteenth century as something quite as splendid, brief, unique, and ultimately abandoned as the outburst of art at the Renaissance. Few people realize that the general habit of fiction, of telling tales in prose, may fade, like the general habit of the ballad, of telling tales in verse, has for the time faded. Few people realize that reading and writing are only arbitrary sciences like heraldry." All that he suggests, even any part of it, seems to us, if not impossible at least incredible, but it is as well to remind ourselves that all things mundane pass, and that what we to-day think of as final, is not necessarily or even probably so. And so after the splendid assurance of the mid-Victorian period, that everything was to be known the day after to-morrow if not sooner, comes the reaction of to-day. Of this we have recently been told in a magistral address, the great tendency, the "characteristic of the promising, though perturbing period in which we live," is "rapid progress,

combined with fundamental skepticism,"* intra-scientific skepticism be it understood, skepticism as to what science can really insist upon, rather than skepticism of things outside science.

It is perhaps not wonderful that with the great burst of scientific knowledge which marked the second half of the nineteenth century, there should arise the idea that science could and would prove the key to all mysteries. When one passes even a few of them under review, the achievements of science are marvellous beyond all description. Look at the immensities of the universe. It takes light one second to travel one hundred and eighty-six thousand miles, and the distance between the sun and the earth being more than ninety-two million eight hundred thousand miles, every sunbeam has spent eight minutes or thereabouts on its journey. It would take an express train, travelling sixty miles an hour and never stopping day or night for coal or water, one hundred and seventy-five years to make that journey. Yet it is a mere trifle to the distances known to exist amongst the stars. Everybody knows the Pole Star by sight. Let any person look at it on his fortieth birthday. The beam which meets his eye, left that star at about the moment the forty year old spectator was first making his entry into this vale of tears. Yet even this again is a trifle if the calculations of astronomers are correct, who tell us that the extreme limit of the stellar system consists of a star whose light takes thirty thousand years to reach us, travelling, though it does, at

* Lodge, *Continuity*, p. 7.

the terrific, the inconceivable rate which I have mentioned. Yet over these incredible distances science exercises her reign, weighing, measuring, analyzing the composition of the heavenly bodies, estimating their orbits, and foretelling with unerring accuracy their movements in the future.

Or look again at the smallest things we know of. The living cell is a very small and, it used to be thought, a very simple thing. Yet the more we know of it the less simple we find it. The writer who said that every cell was full of machinery as complicated and as great as that which is contained in a "Great Eastern," in no way exaggerated the state of affairs. He was not thinking of the further complications which have to be considered when we get to much smaller things than the cell, smaller even than the molecules of the chemical substances which build up the cell, when in fact we arrive at what, but a few years ago, was thought to be the ultimate limit of indivisibility, the atom. For the atom is now said to be made up of electrons, or units of electricity, positive and negative electrons. On this hypothesis the oppositely charged electrons are to be thought of "as flying about inside the atom, as a few thousand specks like full stops might fly about inside a room; forming a kind of cosmic system under their strong mutual forces, and occupying the otherwise empty region of space which we call the atom—occupying it in the same sense that a few scattered but armed soldiers can occupy a territory—occupying it by forceful activity not by bodily bulk."*

* Lodge, *Modern Views of Matter*, p. 11.

We were recently considering the awful distances over which science, under the form of astronomy, exercises her sway. Some of the incredible minuteness of the objects with which she also concerns herself, will be gained by learning that the molecules of hydrogen, in which the electrons fly about like full-stops in a room, are so small that it would require about two million of them placed in a row to occupy one twenty-fifth of an inch, and that fifteen thousand million, million, million of them would weigh—one grain. Of all these things great and small science takes cognizance, and of all of them she can tell us much, more and more every day, new vistas of knowledge constantly opening before her inquiries. But there is one thing which she cannot tell us now or ever, nor can pretend to tell us. She presents to our knowledge a universe composed of matter, and that matter everywhere in motion. But she cannot tell us how that matter came into being, or how it came to be in motion. This limitation of science is of course recognized by everybody. We Catholics, in common with all Christians, say that God Almighty, existing from all eternity, created matter, and endowed it with the wonderful properties which it possesses. It is at least a simple and a sufficient theory. I am not going to deal further with it now; but let us for one moment look at the alternative. If it is not as we believe, then matter is eternal, and it is sentient or alive, and does all these wonderful things by its own powers.

In a remarkable and nowadays too-little read

book by two very eminent Scotsmen—I allude to *The Unseen Universe* by Balfour Stewart and P. G. Tait—it is maintained that the only reasonable and defensible alternative to their hypothesis, namely, the existence of a Creator, "is the stupendous pair of assumptions that visible matter is *eternal*, and that IT IS ALIVE." And they continue, "If anyone can be found to uphold notions like these (from a scientific point of view), we shall be most happy to enter the lists with him."* Yet these assumptions which they regard as a *reductio ad absurdum*, are actually put forward, not so far as I am aware by physicists, who make matter their particular study, but by biologists or by some biologists. At any rate, in the last analysis to this alternative, all such theories as those of an *anima mundi* or immanent god, all pantheistic ideas in fact really reduce themselves.

Apart from any other arguments which can be brought forward, and taking it for the moment as a mere working hypothesis, I think our theory is a more reasonable one than its rival. I can understand the position of a man who says, "I neither know nor can I know about these matters." That was the position of Huxley, and still is the position of many, though I think not of so many as was once the case. But the "matter-alive" view I own baffles me completely. It appears also to have baffled many if not all of the physicists who have studied it, like those from whom I have quoted. One of the greatest of physicists, if not actually the greatest, the late Lord Kelvin, in an address

* Preface to second edition. (Italics and capitals as original.)

to the students of University College, London, having considered these alternatives of which I have been speaking, proclaimed his belief that "science positively affirmed creative power." And certainly it is to the physicists that we must go if we are to get information as to the properties and possibilities of matter. Science cannot tell us how things began.

Professor Ward somewhere remarks that many explanations are excellent once one has got inside a system ; but they do not explain the system itself. But science is still further limited, for there are a whole range of things with which she has nothing to do, and can have nothing to do, since, as we have lately been told by Mr. Balfour, "Science depends on measurement, and things not measureable are therefore excluded, or tend to be excluded from its attention. But life and beauty and happiness are not measurable." I do not press this part of the argument further here. What I want to emphasize is this, that science has its own corner—a large one—but there are other corners ; that science cannot tell us anything about the other corners, any more than the other corners can tell us about science. Finally, that science admittedly cannot give us any convincing answer as to how there come to be any corners at all. All this has been long and well known, and fully recognized by writers of the first importance.

In connection with æsthetic enjoyments, Huxley was obliged to describe such things as the enjoyment of music, of art, of scenery, which cannot be shown to be, or even imagined to be, of any

survival value to human beings, as "gratuitous gifts," that is, as things not in any way due to the action of Natural Selection, or even coming within its province, things altogether outside the ken of science. The same point has been argued in connection with the lower animals. The late Professor Hutton,* of whose early criticisms Darwin spoke in such high terms, claims that the song of birds must be considered from the same point of view. "The song of birds," he writes, "apart from their calls, is also due to a sense of pleasure. Several of the forest birds of New Zealand sing softly to themselves, and it is necessary to be very near them to hear them. This is, probably, the primitive style of bird melody, and the loud-throated thrush and skylark came later. All these songs are the result of pure enjoyment; there is nothing useful in them, so they cannot be due to Natural Selection."

And with regard to ethics, we have the confession of Herbert Spencer, in the days when it was really dreamt that science was to explain everything. At the conclusion of the second volume of his *Principles of Ethics*, he tells us that he found his "satisfaction somewhat dashed by the thought that these new parts fall short of expectation. The doctrine of evolution has not furnished guidance," he adds, "to the extent that I had hoped." As a matter of fact these conclusions might easily have been anticipated, had it been remembered that science can only take into consideration a group of experiences, not the whole sum of

* *Lesson of Evolution*, p. 167.

experiences. These limitations of science, this inability to account for the commencement of things, this incapacity to touch certain spheres of experience in any way, may well lead us to consider whether those who thought that the *Argument from Design* had completely perished were right in their supposition. For it is, of course, with the *Argument from Design* that I am primarily concerned, and to its present position I will now direct my remarks.

Let us commence by taking one out of the many instances of design which are to be found in the pages of Paley's work. In his thirteenth chapter he deals with the tongue of the woodpecker, which he says "is one of those singularities which nature presents us with when a singular purpose is to be answered. It is a particular instrument for a particular use: and what, except design, ever produces such?" Then he proceeds to describe the tongue and its purpose, and asks, with, one might imagine, a prescient eye on the Lamarckian theory which was to come, "Should it be said, that, by continual endeavours to shoot out the tongue to the stretch, the woodpecker's species may by degrees have lengthened the organ itself beyond that of other birds, what account can be given of its form, of its tip? how, in particular, did it get its barb, its dentation? These barbs, in my opinion," he concludes, "wherever they occur, are decisive proofs of mechanical contrivances."

It is clear what kind of argument underlies these words, and many others of a like kind in the same book. It is the argument of the watch found

on the heath. The object whether watch or woodpecker's tongue was designed obviously and ingeniously for a certain purpose. It must have been designed for that purpose by an intelligent being. In the case of the watch this was the watchmaker; in the case of the tongue it must have the Author and Creator of nature.

This argument, as we have already seen, appeared to be shattered by Darwin's views. He himself remarks, in connection with the instance I have selected from Paley,* "I can see no reason why he (*i.e.*, Asa Gray, with whom the letter deals) should rank the accumulated variations by which the beautifully adapted woodpecker has been formed as providentially designed."

What shattered or appeared to shatter the *Argument from Design?* The formulation of the theory of Natural Selection. And how did that shatter or appear to shatter it? Because that theory taught and seemed to prove that the contrivances on which the *Argument from Design* had appeared to rest, were the result of a process of Natural Selection exercised amongst a myriad of variations constantly arising in living things, the selection being effected by the process of the elimination of the less fit, and the survival of the fitter, that is of those who by virtue of those favourable variations were better able to succeed in the struggle for life. These views thus formulated undoubtedly seemed to prevail for a time, and in the opinion of many at the time, and perhaps of many even to-day, the *Argument from Design* disappeared as one

* *Life and Letters*, vol. i, p. 314.

unworthy of the consideration of reasoning persons.

But has it been shattered or has it not, as restated in face of the present attitude of science, really acquired a greater force than it possessed in the days of Paley, the days before the *Origin of Species* had appeared? This is the question which I desire to consider, and in doing so I do not pause to argue as to whether the theory of Natural Selection is or is not true. There are at least three well-marked schools of opinion on that head. There are those who deny its effectiveness in evolution altogether, a small but existent band. There are those who look upon it as the main, even the sole and sufficient factor of evolution, a larger, a more important, but perhaps decreasing band. And there is the middle party which, whilst regarding Natural Selection as an agent, perhaps a very important agent in evolution, sees that its power is limited, and probably inferior to other factors, such, for example, as isolation.* Let us assume the theory in question to be true, and consider how it bears upon the argument with which I am dealing.

It is curious that it should be necessary to make the observation, but it is necessary to point out that Natural Selection cannot *cause* anything, and this because it cannot *cause* any variation. It is also curious that this fundamental misconception often made, at least by implication, to-day should have been made in Darwin's own time, and corrected by Darwin himself in later editions

* This question is dealt with more fully in the succeeding article.

of his book, in which he says : " Some have even imagined that Natural Selection induces variability, whereas it implies only the preservation of such variations as arise, and are beneficial to the being under its conditions of life." If Natural Selection cannot cause a variation—as, of course, it cannot—it is quite clear that, if it is an explanation at all, it is not a complete explanation. But is it even a partial explanation, and if so what actually does it explain ?

Driesch, in those valuable lectures which he delivered in a Scottish University under the Gifford Trust, points out that Natural Selection

" can only eliminate what cannot survive, what cannot stand the environment in the broadest sense, but that Natural Selection never is able to create diversities. It always acts negatively only, never positively. And therefore it can explain [he continues], if you will allow me to make use of this ambiguous word—it can ' explain ' only why certain types of organic specifications, imaginable *a priori*, do *not* actually exist, but it never explains at all the existence of the specifications of animal and vegetable forms that are actually found. In speaking of an ' explanation ' of the origin of the living specific forms by Natural Selection, one therefore confuses the sufficient reason for the non-existence of what there is not, with the sufficient reason for the existence of what there is. To say that a man has explained some organic character by Natural Selection is, in the words of Nägeli, the same as if someone who is asked the

question, 'Why is this tree covered with these leaves?' were to answer, 'Because the gardener did not cut them away.' Of course that would explain why there are no more leaves than those actually there, but it would never account for the existence and nature of the existing leaves as such. *Or [he concludes] do we in the least understand* why there are white bears in the Polar Regions if we are told that bears of other color could not survive?"*

Darwin himself recognized this fact, and in one of his letters says, "Talking of 'Natural Selection'; if I had to commence *de novo*, I would have used 'natural preservation.'"†

"Natural Selection," says de Vries,‡ "acts as a sieve; it does not single out the best variations, but it simply destroys the larger number of those which are, from some cause or other, unfit for their present environment. In this way it keeps the strains up to the required standards, and in special circumstances may even improve them."

It cannot originate variations: that is the first point to which we have to direct our attention. And in the second place it cannot do every thing in choosing and shaping the variations with which it is confronted. What causes the variations? That is the kernel of the whole matter, and it is one on which science at present, it must be confessed, can shed but little light. It is no good saying that there is an inherent tendency in all living

* *Science and Philosophy of the Organism*, vol. i, p. 262.
† *Life and Letters*, vol. ii, p. 346.
‡ *Darwin and Modern Science*, p. 70.

things to vary : that explanation is purely verbal. To say that a thing inheres in, or sticks in, something else, does not explain why it inheres or how it got stuck there.

Of course there is the question of the action of the environment as to which so much doubt exists to-day. Of this it may be said that even if we grant it all the influence which its warmest advocate could ask, it still does not explain the question, because it does not explain how living matter acquired the property of responding to the influence of the environment. We have seen that Darwin in set terms disclaims the efficacy of Natural Selection as a cause of variations, and in face of the fact is it not extraordinary to find a man of science stating that " we must assume Natural Selection to be the principle of the explanation of the metamorphoses, because all other apparent principles fail us, and it is inconceivable that there should be another capable of explaining the adaptation of organisms without assuming the help of a principle of design." Yet in such terms—illogical in the extreme, so it seems to me, does Weismann address himself to the solution of the difficulty. And, in so doing, he seems to me to throw a light upon the point with which we are concerned. If, he says, Natural Selection cannot explain the matter, then we must have recourse to the only other possible alternative—that, to him, appalling alternative—the principle of design.

We need not hesitate to grant that these are the two alternatives with which we have to do. Now let us for a moment suppose that Natural

Selection does everything that its most ardent *worshippers claim for it, more, far more, than* Darwin, its original describer claimed for it, can it dispense with design? That is the question to which we may well address ourselves. Just let us recall for a moment what is claimed for the alternative; what has to be accounted for by those who deny the existence of an Intelligent Author of the universe. The world, so science assures us, at a certain date in the past, was a mass of nebulous matter at a terrifically high temperature. Slowly and with vast convulsions and cataclysms, it cooled down. Then by some chance mixing together of some nitrogen, hydrogen, oxygen, carbon, and other elements, in some manner hitherto undiscoverable by, and even unimaginable by, modern chemists, the lowest form of living organism emerged—the offspring of the blindest kind of chance, yet endowed somehow or another with the marvellous power of propagating its kind, and, more, with a tendency to vary fortuitously in all directions. Then the law of Natural Selection, also the result of blind chance, sprang into existence without any Lawgiver to lay it down. By this simple process of extinguishing the disadvantageous variations, Natural Selection developed out of the come-by-chance Protozoan all the forms of animal and vegetable life which have flourished on this earth, or which now astonish us by their multitude and variety. Finally it brought forth the head and crown of things—man. And more, far more, the brain of man.

And what does that mean? *Hamlet, Paradise*

Lost, the *Differential Calculus*, the music of Handel, the paintings of Botticelli, internal combustion engines, wireless telegraphy, all the poetry of a Wordsworth, all the wonderful inventions of a Kelvin. All these things and a thousand more as wonderful, the Law of Natural Selection without a spark of intelligence behind it—this perfectly aimless action of physical forces—all these things it has accomplished. This is the demand which is made upon our powers of belief by those who deny the existence of an intelligent Author of the universe, and attempt to put forward an explanation of the existence of things as they are. Natural Selection, if it be a law of nature, as we are assuming it to be, must be either the product of mechanical forces acting at random, or it must proceed from an intelligent Lawgiver. There is no middle term, since, as we have seen, there is in the last abstraction nothing between believing in a Being—a Lawgiver—Who is something in Himself apart from the world, and believing in a mere abstraction from, or generalization of, natural laws or processes, and that, apart from a Lawgiver, means nothing more than blind chance.

In a letter published in the London *Times*, in connection with the alternatives just discussed, Lord Kelvin, in maintaining that there was no middle choice open to us, narrated a conversation which he had once had with the great chemist Liebig. When walking with him in the country, Sir William Thomson, as he then was, asked Liebig whether he believed that the grass and the flowers which were all round them grew by chemical

forces alone. Liebig's reply was that he could no more believe that than he could believe that a botanical work, describing these objects, could be produced by mere chemical forces. It is indeed a little difficult to see how anyone can deliberately embrace the blind chance alternative. In the discussion at Berlin between Father Wasmann and a number of materialistic opponents which excited so much interest a few years ago, this point was very clearly put by Professor Plate, an avowed upholder of monistic and materialistic views. He said: "The monist asserts nothing about the nature of God, but limits himself to the laws of nature. These laws are, indeed, the only things that we can establish with certainty; with regard to what underlies them there are many different opinions, and we monists are not all agreed on the subject. Personally," he continues, and this is the most important part of his address, "personally, I always maintain that, if there are laws of nature, it is only logical to admit that there is a Lawgiver. But," he concludes, "of this Lawgiver we can give no account, and any attempt to give one would lead us into unfounded speculations."[1] Such is his view. What at any rate emerges from it is the *Argument from Design* in a new form. Instead of the argument to the Artificer from the artifice, we have the argument to the Lawgiver from the law under which the artifice has constructed itself.

It certainly is not, at least in my opinion, a weaker argument, rather one stronger and possessed of a greater grandeur than the old argument.

"If evolution [says Father Boedder]* be the true explanation of the existing order of the cosmos and this evolution is due to the gradual working out to their final issues of laws inherent in matter from the commencement, then the question whether this existing order be due to intelligence or not, is not solved, but merely pushed back. In the achievements of human industry, a self-constructing machine would be taken to imply not comparative absence of skill and contrivance in its maker, but a higher exercise of these qualities; and the same will have to be said of the machine of the cosmos. The more its order is due to an evolution which is the outcome of the action of fixed laws inherent from the first and tending definitely towards the final result, the more striking is the manifestation which it bears upon its face."

"Know, silly child," said Mother Carey to the fairy who had made a butterfly, "know, silly child, that anyone can make things, if they will take time and trouble enough; but it is not everyone who, like me, can make things make themselves."† Now all that we are learning daily from science, perhaps most of all from biology, under the influence of the remarkable facts first discovered by the Augustinian Abbot Mendel, does certainly seem to leave no doubt as to the existence of those orderly series of occurrences which we call "laws of nature." If such there be they must either be the result of the ordainments of a Lawgiver or they

* *Natural Theology*, p. 166.
† Kingsley, *Water Babies*, chap. vii.

must be the results of blind chance. It is the same problem which confronted Paley, stated in a somewhat different manner. He was obliged to consider whether the watch came to be as it was by blind chance, or because it had been made to tell the time by an intelligent artificer. We are asked to decide whether the laws under which life works out its ends, are the result of blind chance or come from a Lawgiver. In their essence the two inquiries are identical, and those who would have elected for blind chance under the Paleyian dilemma will do so now, whilst those who think that law and order and progress are inexplicable, not to say impossible, without a Lawgiver and an Orderer, will hold the conclusion at which Paley arrived, that the world shows forth its Creator in unmistakable language. Many other issues, all of them interesting, arise in connection with this matter, but with none of them can I find space to deal. What I have been anxious to show is, that the argument which held the field before the storm, when the lake was comparatively calm, now that the tempest has raged over it, still remains, restated as we may suppose the waters of the lake to have rearranged themselves during the commotion to which they were subjected, but essentially the same, and the same because founded upon what we cannot but regard as being the Eternal Verities.

II: DARWIN AND THE THEORY OF NATURAL SELECTION

IN no case is the philosophical rule that terms must first be defined before argument can be entered upon more valuable, indeed essential, than in the case of what is called, though often incorrectly, Darwinism. As has been pointed out time and again, there are three or four conceptions included under this name, only one of which really merits the appellation. The idea which first rises to the mind of the ordinary person when Darwinism is mentioned is that of transformism—or the derivation of one species of living things from some other species, putting the doctrine in its crudest possible form for the purpose of easy recognition. Of this theory Darwin neither was nor ever claimed to be the parent. Apart from the long list of non-Catholic authors cited in the various books and articles dealing with Darwin's predecessors, there are also, as Mivart* and Wasmann† have shown, a number of Catholic authorities, including St. Augustine, St. Thomas Aquinas, Suarez, Cornelius á Lapide and that very distinguished Jesuit, Athanasius Kircher (who first suggested the germ-origin of disease), who have alluded with approval to a transformistic explanation of nature as we know it. What Darwin

* *Genesis of Species*, pp. 303 et seq.
† *Modern Biology and the Theory of Evolution*, p. 276 and Note.

really did was, in the first place, to suggest a method, or methods, by means of which he thought transformation might have taken place; and, secondly, and still more importantly, to make the theory of transformation popular.

"To the end of time, if the question be asked, 'Who taught people to believe in Evolution?' the answer must be that it was Mr. Darwin. This is true, and it is hard to see what palm of higher praise can be awarded to any philosopher." So writes one of Darwin's most serious critics and opponents.* Transformism, then, is not Darwinism, though Darwin was a transformist.

In the next place, there is the monistic philosophy, often called Darwinism, and trumpeted as such by little (and usually misleading) manuals and by Haeckel in his much-lauded but scientifically discredited philosophical works. "We should never have reached this supreme general conception" (of the so-called "all-embracing 'Law of Substance'")† "if Charles Darwin—a 'monistic philosopher' in the true sense of the word—had not prepared the way by his theory of descent by natural selection, and crowned the great work of his life by the association of this theory with a naturalistic anthropology."‡ After this blast of

* Samuel Butler, *Evolution, Old and New*, Preface to 2nd ed. (1882).

† For a criticism of which see Sir Oliver Lodge's *Life and Matter*. "He writes (*i.e.*, Haeckel) in so forcible and positive and determined a fashion from the vantage ground of scientific knowledge, that he exerts an undue influence on the uncultured amongst his readers, and causes them to fancy that only benighted fools or credulous dupes can really disagree with the historical criticisms, the speculative opinions and philosophical, or perhaps unphilosophical, conjectures thus powerfully set forth," op. cit., p. 135.

‡ Article in *Darwin and Modern Science*, p. 151.

the Haeckelian horn, it may be well to listen to the words on the same subject of another man of science, Professor Dwight: "We have now the remarkable spectacle that, just when many scientific men are of accord that there is no part of the Darwinian system that is of any very great influence, and that as a whole the theory is not only unproved but impossible, the ignorant, half-educated masses have acquired the idea that it is to be accepted as a fundamental fact. Moreover, it is not to them an academic question of biology, but, as the matter has been presented to them, it is a system: to wit, the monistic system, of philosophy. Thus presented it undeniably is fatal, not only to all revealed religion, but to any system of morals founded on a supernatural basis."* It is perhaps worth while noting that Darwin himself never claimed the position of a "monistic philosopher." No doubt he grew more agnostic as the years of his life rolled by, but to the end of that life he never expunged from its pages the well-known passage with which the *Origin of Species* terminates, though he modified that passage in his first draft, and again, in a very remarkable manner, in the second edition. Here is the passage itself: "There is grandeur in this view of life, with its several powers, having been originally created by the Creator into a few forms *or into one*; and that, whilst this planet had gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful

* *Thoughts of a Catholic Anatomist*, p. 6. By Thomas Dwight, M.D., LL.D., Parkman Professor of Anatomy at Harvard.

and most wonderful have been, and are being evolved."* Now as to the alterations made, which are not uninstructive. In the first place, the words which I have italicized were added in pencil, in the first draft, showing that Darwin was at that time, at least, hesitating between a monophyletic and a polyphyletic theory; and, in the next place, the words "by the Creator" were introduced in the second edition.†

It is obvious that it is unfair to Darwin to hide the monistic derivations which some have made from his works under the ægis of his name.

There is another aspect of Darwin's life-work which might be much more fairly spoken of as Darwinism, though, as a fact, few, if any, would speak of it in that way, and that is the great mass of positive contributions to science which must ever remain an abiding honour to his name. Of such are his works connected with the Voyage of the "Beagle," the Volcanic Islands, the Monograph on the Cirripedes, the fascinating volumes on Orchids, Climbing Plants, Earthworms and the like. These are works which excite much less popular interest than his more theoretical treatises, for the reason that they are mostly related to positive science, and only in a minor degree to philosophical theory. When Driesch‡ proclaims that Darwinism "is a matter of history, like that other curiosity of our century, Hegel's philosophy,"

* *Origin of Species*, (6th ed.), p. 429.

† *The Foundation of the Origin of Species*, p. 254, Note and p. 53.

‡ The quotations are from the *Biologisches Zentralblatt*, the first 1896, p. 355; the second 1902, p. 182.

and continues that "both are variations on the theme, 'How to take in a whole generation,' and neither is very likely to give ages to come a high opinion of the latter part of our century," or when he states that "for men of clear intellect, Darwinism has long been dead"; when Dennert entitles a treatise, "Vom Sterbelager des Darwinismus," these writers are in no way alluding to the group of works of which mention has just been made. These rather violent and, in the present writer's opinion, exaggerated statements have been quoted in order to exhibit the antipodes of scientific opinion to the attitude assumed by Haeckel, Weismann and their schools. What the first-named writers are alluding to is to Darwinism proper, namely, the Theory of Natural Selection and the other minor theories associated with Darwin's attempt to explain the mechanism of transformation. It is to the first and most important of these that we must now turn our attention. When doing so, it is above all things important to recall to one's mind the title-page of the book in which Darwin's views were first given to the world. It is probable that many, if not most, persons suppose that the title of the book is the *Origin of Species*, *tout court*, but what it really is may here be set down: *The Origin of Species by means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.* This title contains two phrases, "fatal phrases which have become almost household words,"† as Professor

† *Evolution and Adaptation*, p. 107. The reader in search of a detailed criticism of Darwinian views by an acknowledged scientific authority may be referred to this work. (Macmillan, 1903.)

Morgan puts it, viz., "Natural Selection" and "The Struggle for Life," phrases of which everybody has heard, the latter having even passed into Parisian argot. The object of the present article is to take stock of the present state of scientific opinion as to Natural Selection, and this may best be effected by composing a catena of quotations from recognized scientific authorities, connected by such amount of running commentary as may seem useful to bind the words of others into a compact bundle.

Darwin's theses were, firstly, that variations do occur; secondly, that "variations useful in some way to each being in the great and complex battle of life" occur also and "in the course of many successive generations"; and, finally, that if these do occur, "can we doubt (remembering that many more individuals are born than can possibly survive) that individuals having any advantage, however slight, over others, would have the best chance of surviving and of procreating their kind?" *

It would be travelling far beyond the scope of this article to attempt any account of the history of the reception of this theory. All the educated world knows about it, and all can learn about it in numberless biographies and other works. It is the state of present day opinion with which we are concerned, and such opinions may be segregated into three groups. There are those who firmly adhere to the Darwinian view in all its

* *Origin of Species*, etc. (6th ed.), from which all quotations in his article are taken, p. 63.

pristine glory. Amongst these may first be cited the distinguished co-emitter of the theory, A. R. Wallace. "Whatever other causes have been at work, Natural Selection is supreme, to an extent which even Darwin himself hesitated to claim for it."*

Then there is Lankester, who, in his Presidential Address to the British Association in 1906, committed himself to the statement that "in looking back over twenty-five years, it seems to me that we must say that the conclusions of Darwin as to the origin of species by the survival of selected races in the struggle for existence are more firmly established than ever."

Then, in the same group, we find, of course, the name of Weismann, and to his statements some small amount of space must be devoted. His latest pronouncements have been made in an article in the volume published by the University of Cambridge at the Darwin Centenary.† When he says "that selection is a factor, and a very powerful factor, in the evolution of organisms can no longer be doubted" (p. 61), he is going further than all scientific men would go, as will be seen by later quotations, but unquestionably he would find many, even of moderate views, very nearly, if not quite, in agreement with him. But when he proceeds to state that "the principle of selection solved the riddle as to how what was purposive could conceivably be brought about

* *Darwinism* (1889), p. 444. See also his latest work, *The World of Life* (1911), pp. 124 et seq., and especially the instructive instance on pp. 127-8.

† *Darwin and Modern Science.*

without the intervention of a directing power, the riddle which animate nature presents to our intelligence at every turn, and in face of which the mind of a Kant could find no way out, for he regarded a solution of it as not to be hoped for" (p. 21), one is entitled to ask whether he has ever heard of the series of difficulties enumerated, for example, by Driesch, and later on to be dealt with more fully and, if so, how he proposes to get over these difficulties by the aid of selection. The real explanation of Weismann's attitude appears (not for the first time by the way) in another part of the same article, where, after admitting that "we cannot bring formal proofs of it (selection) *in detail*," he goes on to say that "*we must accept it because the phenomena of evolution and adaptation must have a natural basis and because it is the only possible explanation of them*" (p. 61; the italicized words are so printed by the author). It is now nearly twenty years ago since Weismann's controversy with Herbert Spencer on the All-Sufficiency of Natural Selection. In the course of that discussion Weismann very clearly explained why he believed in natural selection, and we must leave it to our readers to decide whether the grounds upon which he founded his belief were solid and unassailable. "We must assume," he wrote,* "natural selection to be the principle of the explanation of the metamorphoses, because all other apparent principles of explanation fail us, and it is inconceivable that there should be another capable of explaining the adaptation of organisms *without*

* *Contemporary Review* (1893), italics again the authors.

assuming the help of a principle of design." This is an excellent example of the *parti pris* of that dogmatic Darwinism against which Driesch very properly protests, pointing out that whilst Darwin himself was anything but dogmatic, Darwinism is dogmatism in one of its purest forms.

It must be perfectly clear to any candid student that arguments based upon foundations such as this are not worth the paper they are written on until it can be proved, as in fact all must admit that it never can be proved, that a principle of design is demonstrably non-existent.

In opposition to the opinions just dealt with are those of a very large and, it would certainly seem, increasing number of scientific men of the very highest standing. Those who are desirous of making a more detailed study of their opinions may be referred to a very useful work, to which the writer of the present article wishes to make his own acknowledgments.* Professor Kellog, of Leland Stanford (jun.) University, who is the author of this book, belongs to the school of Plate, whose apologia for Darwinism is described by Driesch as "scarcely more than a funeral oration in accordance with the principle, *de mortuis nil nisi bonum.*" From this it will be understood that Professor Kellog is in no way bigoted against Darwinism, though it can scarcely be said that his attitude towards religion could be described in the same way. At any rate his book, if only on account of the careful digests of the literature given as an appendix to each chapter, is one of

* *Darwinism To-Day* (1907).

very great value to any person working at higher biological problems, and we now proceed to exemplify that fact by making some quotations from it, premising that Professor Kellog, whether he approves of them or not, is very fair in his representation of all opinions expressed by persons worthy of consideration in scientific studies. We may profitably commence by considering in a general way the kind of scientific opinion which at present shows itself more or less antagonistic to Darwinian views, before we deal particularly with the more prominent opponents and their opinions. "There has been," Professor Kellog writes, "from the day of the close of the first great battle to the present moment a steady and culminating stream of scientific criticism of the Darwinian selection theories. In the last few years it has, as already mentioned in the preface and introductory chapter of this book, reached such proportions, such strength and extent, as to begin to make itself apparent outside of strictly biological and naturo-philosophical circles. Such older biologists and natural philosophers as von Baer, von Kölliker, Virchow, Nägeli, Wigand and Hartmann, and such other writings in the nineties and in the present century as von Sachs, Eimer, Delage, Haacke, Kassowitz, Cope, Haberlandt, Henslow, Goette, Wolff, Driesch, Packard, Morgan, Jaeckel, Steinmann, Korschinsky and de Vries are examples which show the distinctly ponderable character of the anti-Darwinian ranks. Perhaps these names mean little to the general reader; let me translate them into the professors of zoology, of botany,

of palæontology and of pathology in the universities of Berlin, Paris, Vienna, Strasburg, Tübingen, Amsterdam, Columbia University, etc." (p. 26). These opponents do not, however, stand on one common platform. Some of them wholly deny that natural selection has any kind of influence in or capacity for the formation of species. These are at the opposite pole of scientific thought to a man like Weismann, who practically regards natural selection as being all-powerful in this respect.

To this, the second of the three groups alluded to a few lines above, must be added the third class, which consists of men who, whilst not denying that natural selection has an influence, perhaps even a potent influence, in the formation of species require the acceptance of stringent limitations of that power. These altogether deny its all-powerfulness, though they do not deny its powerfulness.

According to Professor Kellog, the palæontologists, as a whole, must be classed with the root-and-branch opponents, if it be true, as he says—and his evidence, as has been pointed out, is not based against natural selection—that this important group of scientific men "believe practically as a united body, that variation has followed fixed lines through the ages; that there has been no such unrestricted and utterly free play of variational vagary as the Darwinian natural selection theory presupposes" (p. 33). To these may be added two very eminent botanists, Nägeli, who "believes that animals and plants would have developed about as they have, even had no struggle

for existence taken place, and the climatic and geologic conditions and changes been quite different from what they have been" (p. 273); and, still further in opposition to the neo-Darwinian position, Korschinsky, a Russian, who holds that "the struggle for existence, and the selection that *goes hand in hand with it, constitute a factor* which limits new forms and hinders further variation, and is, therefore, in no way favourable to the origin of new forms. It is a factor inimical to evolution." (p. 335).

So much for the two poles of opinion: Weismann is quite sure that natural selection can do everything, because, if it cannot, one must believe in a Divine Guide; Nägeli thinks that natural selection can do nothing, and, it may be parenthetically remarked, has put forward a theory of orthogenesis, which depends upon "a principle of progressive development (Vervollkommungsprinzip), a something inherent in the organic world which makes each organism in itself a force or factor making towards specialization, adaptation, that is, towards progressive evolution" (p. 278); whilst Korschinsky, whose theory of evolution is very similar to that just described, will have nothing at all to do with natural selection as a factor in progressive evolution, since he regards it as a hindrance instead of an assistance. It would certainly, however, be true to say that by far the greater number of those whose names are included in Kellog's list, given above, hold the middle opinion. Nor would it be going too far, we think, to say that the majority of biologists

would agree with Fr. Wasmann* that natural selection is "indispensable as a subsidiary factor, but only a *factor*," and with Bateson† that "by the arbitrament of natural selection all must succeed or fall," but that its scope "is closely limited by the laws of variation."

It is closely limited by the laws of variation. In what directions is it limited? What actually can it do? What cannot it do? All these are questions which have been discussed again and again by men of science ever since the Theory of Natural Selection first saw the light. It will be quite worth while devoting a few minutes to the consideration of this point, since it leads up to and controls the next step of the argument in which we are engaged.

"Natural Selection may explain the survival of the fittest, but it cannot explain the arrival of the fittest!" (p. 89). Thus neatly summed up by a friend of Kellog's, the question might almost be left. But it will be well to particularize. Driesch, in his masterly lectures, given under the Gifford Trust, and a fit companion of the admirable courses in that series by Professor Ward and by Lord Haldane, criticizes most carefully‡ the claims of natural selection, and tells his auditors that‖ "it always acts negatively only, never positively. And, therefore, it can 'explain'—if you will allow me to make use of this ambiguous word—it can

* *The Problem of Evolution*, p. 42.

† Mendel's *Principles of Heredity*, p. 289.

‡ As has been already pointed out in the preceding article. See p. 25.

‖ Driesch, *Science and Philosophy of the Organism*, p. 262.

'explain' only why certain types of organic specifications, imaginable *a priori*, do *not* actually exist, but it never explains at all the existence of the specifications of animal and vegetable forms that are actually found. In speaking of an 'explanation' of the origin of the living specific forms by natural selection, one therefore confuses the sufficient reason for the non-existence of what there is not, with the sufficient reason for the existence of what there is." And again, "that dogmatic Darwinism has been found to be unable to explain every kind of mutual adaptations, *e.g.*, those existing between plants and insects; that it can never account for the origin of those properties that are indifferent to the life of the bearer, being mere features of organization as an arrangement of parts; that it fails in the face of all portions of organization which are composed of many different parts—like the eye—and nevertheless are functional units in any passive or active way; and that last, not least, it has been found to be quite inadequate to explain the first origin of all newly-formed constituents of organization, even if they are not indifferent: for how could any rudiment of an organ, which is not functioning at all, not only be useful to its bearer, but be useful in such a degree as to decide about life or death."* Finally, as regards the views of the powerful writer from whom the last two quotations have been made: "It is altogether impossible to account for the restitutive power of organisms by the simple means of fluctuating variation and natural selection

* Driesch, *Science and Philosophy of the Organism*, p. 266.

in the struggle for existence. Here we have the logical *experimentum crucis* of Darwinism." * It seems more than a little difficult to understand how, save on the Nelsonic principle of applying the telescope to the blind eye, such a claim as that quoted above, that the riddle of existence is solved by natural selection, can be made in the face of such difficulties as those enumerated in the last two quotations, and often urged during the years which have passed since Darwinism first took the field. Let us, however, look a little further into the views of the middle or moderate school of thought. Bateson, the champion of Mendelism, and one of the first biologists in England, states that " to begin with, we must relegate Selection to its proper place. Selection permits the viable to continue, and decides that the non-viable shall perish ; just as the temperature of our atmosphere decides that no liquid carbon shall be found on the face of the earth : but we do not suppose that the form of the diamond has been gradually achieved by a process of Selection. So, again, as the course of descent branches in the successive generations, Selection determines along which branch Evolution shall proceed, but it does not decide what novelties that branch shall bring forth. '*La Nature contient le fonds de toutes ces variétiés, mais le hazard ou l'art les mettent en œuvre*,' as Maupertuis most truly said."†

And, finally, as far as this point is concerned, the matter is summed up by de Vries, the Professor

* Driesch, *Science and Philosophy of the Organism*, p. 267.

† *Darwin and Modern Science*, p. 96.

of Botany in Amsterdam and the author of the most recent work on Mutation: "Natural Selection acts as a sieve; it does not single out the best variations, but it simply destroys the larger number of those who are, from some cause or another, unfit for their present environment. In this way it keeps the strains up to the required standard, and, in special circumstances, may even improve them."*

So far, then, for the main outlines of the theory. The quotations given abundantly prove at least one point, namely, that scientific opinion to-day is anything but unanimous as to what natural selection can do, or even whether it can do anything at all. It remains to examine certain subsidiary problems arising out of the doctrine, and here again I shall take pains to point out what recent writers have said on each problem as it arises. First of all, then, it is clear that if natural selection is a sieve, it must have something to sift —that is to say, there must be such things as variations to be operated upon. We need not linger over this point, for no one doubts that variations do occur, though, as we shall shortly see, there is considerable difference of opinion as to the kind of variations which really count in connexion with natural selection. The real question is how these variations come about; that is the question of questions in Biology—in fact, as Samuel Butler most acutely observed, "To me it seems that the 'Origin of Variation,' whatever it is, is the only true 'Origin of Species,'"†

* *Darwin and Modern Science*, p. 70.
† *Life and Habit*, p. 263 (1910 ed.).

It seems ridiculous to suppose that natural selection can actually cause the variations on which it is to operate, yet the claims of some of its supporters go as far as that. In the preceding article we have seen that such claims were actually made during the lifetime of Darwin and repudiated by him.

If natural selection cannot cause the variations, can any other external agent cause them? Here we are brought face to face with the great question of the inheritance of acquired conditions, with which it would be quite impossible to deal in this article.* It must suffice to say very briefly that it is as certain as anything can be that mutilations are not inherited; that there is little valid evidence for the inheritance of conditions otherwise acquired, but that there is some evidence that change of environment, extending over a number of generations, may cause the fixation of an acquired variation.

Further, it may, I think, be fairly said that the small amount of positive evidence on this matter does not in any way account for the variations which we know to arise so frequently in the various living things which we see around us. In fact, the impossibility of accounting for these variations by external influences, and the desperate desire to have nothing to do with anything which could savour of a Creative Influence, are the real origin of the mass of unproved and admittedly unprovable assumptions, culminating in his *Biophoridæ*, which have been put forward by

* Some account of this matter and of Weismann's views thereon will be found in a later article, pp. 70, seq.

Weismann. This confession of failure, for that is what it amounts to, to account for variations by external agents is very striking and, in my opinion; conclusive. If external agents fail to account for variation, and it is yet clear that variation does occur, then it follows that the change must be caused by some internal factor. Weismann constructed his complicated edifice of "determinants," "ids," "biophores" and the rest to avoid assuming the principle of design and to provide a "natural" explanation of the internal factor; but, though no one can say that these things do not exist, since they are admitted by their author to be invisible, neither can any person say that they do exist, and as science has only to do with what can be demonstrated, it is clear that, until they are demonstrated, they can have nothing to do with science or science with them.

An internal, inherent force, an "entelechy" (to use Driesch's Aristotelean term), is therefore being postulated to-day by many men of science besides the distinguished writer whose name has just been quoted, and whose excellent series of Gifford lectures are worthy of the most careful study by all persons interested in the higher problems of biology. Nägeli and Korschinsky, as we have seen, not to speak of a number of others, believe that evolution is due to immanent factors in the living cell or the living organism, that these forces work along definite lines and that they cause the variations which we know to occur.* In general

* In the present writer's work, *What is Life?* (Sands and Co.) will be found some account of the support for a vitalistic explanation of life given by men of science.

terms, the orthogenetic theory may be summed up in the words of Korschinsky, as quoted by Kellog: "In order to explain the origin of higher forms out of lower, it is necessary to assume in the organism a special tendency towards progress." Or, again, it may be given in the words of Henslow:* "The origin of Variations in Structure (upon which alone species are based) is due to an inherent power within the plant, by means of which it responds to the direct action of changed conditions of life." Unpalatable as it undoubtedly is to biologists of the type of Weismann and Plate, and in fact all the adherents of the pure materialistic school, there can be no doubt that vitalistic views are daily gaining ground, and that some such vitalistic explanation as those just cited does fit in with and explain the facts as no chemico-physical theory of the day goes. Of course, it may be said that such a theory can no more be demonstrated by the microscope than can Weismann's Biophores. But the two things are not in *pari materiâ*. Weismann's imaginary objects, if they existed, would be actual particles of matter and, therefore, might conceivably be demonstrated by some instrument of powers at present unknown to science—at any rate ought to be theoretically capable of ocular demonstration. Until, therefore, they are demonstrated, they cannot be said to have come within the cognizance of science. The "entelechy," the "vital" or "bathmic force," or whatever else one may care to style it—the inherent power—is not a material object like a

* *The Heredity of Acquired Characters in Plants* (1908), p. 6.

bead of protoplasm, although it may be associated with that bead, and the arguments which may be used concerning it are of quite a different order from those which can be used about the "biophores."

It is open to the opponents of this view to say that the vitalistic hypothesis is unprovable, and that we can know nothing about the matter, and that, in fact, is the attitude of despair assumed by some, at least, of them. Those who believe that the existence of such a force is capable of proof by adequate arguments will find ample corroboration of their view in the closely argued pages of Driesch's work just alluded to.

Returning to the variations which occur in the world of nature, we shall find that some of these are small and some greater—that is to say, that the departures from what may be considered to be the normal type of the form in question are slight or are considerable. Much controversy rages at present as to whether both kinds of variation may be inherited and, if not, which of the two affords the means of transformation or evolution. Darwin himself seems to have pinned his faith to the smaller changes, though there is some ambiguity in his works on the subject, and consequently some difference of opinion amongst his present-day commentators as to his real meaning.*

On the other hand, de Vries† has put forward a theory which has been tentatively advanced by

* On this point, see *Darwin and Modern Science*, pp. 70, 71.

† See his work, *Species and Varieties* (1905), of which an account is given in a later article.

some previous writers, that whilst the minor changes swing backwards and forwards like a pendulum across the mean, no real advance occurs through them. In his opinion, it is only the greater variations, which he calls Mutations, which really count. These occur suddenly; at periods and not continuously; and hence are discontinuous, and thus in harmony with nature, which, as we see it, is also clearly discontinuous. This theory is at present the subject of a very active discussion, and cannot in any way be said to be decided one way or the other.

From what has been said it will clearly be seen that there has been a very remarkable change in scientific opinion during the past twenty-five years, and that that change of opinion, though many would be very loath to admit it, has been away from the materialistic pole and towards its antipodes—the old explanations of Christian philosophy. Further still, some men of science whose minds have not previously been turned in that direction are obviously on the road to the discovery of a Plan and an Author and Guider of Nature. How this is coming about may be judged by a number of utterances, of which one, now to be quoted, by Professor Bateson, one of the most distinguished, as he is certainly one of the most open-minded of modern biologists, is certainly very remarkable.*

"With the experimental proof that variation consists largely in the unpacking and repacking of an original complexity, it is not so certain as

* *Darwin and Modern Science*, p. 101.

we might like to think"—(May one interrupt the Professor for one moment to ask why, when we are discussing scientific problems, we "might like to think" anything, and why in any case might we not like to think that Creation has a Creator and Lord?)—"it is not so certain as we might like to think that the order of these events is not predetermined. For instance, the original 'pack' may have been made in such a way that at the *n*th division of the germ-cells of a sweet pea a colour-factor might be dropped, and that at the $n + n$th division the hooded variety be given off, and so on. I see no ground whatever for holding such a view, but in fairness the possibility should not be forgotten, and in the light of modern research it scarcely looks so absurdly improbable as before." Now, with all reverence, be it said, if there is a Pack, it would seem to follow that there must have been a Pack Maker. In the course of the discussion which followed the series of lectures given by Fr. Wasmann in Berlin, a series which created so much interest on the Continent, and of which the reverberations reached even to the English papers, Professor Plate committed himself to the statement that "if there are laws of nature, it is only logical to admit that there is a lawgiver."* The excellent Jesuit congratulates himself on this admission, but surely it is a truism, if not actually a platitude—a statement which can only be denied by a direct dislocation of the intellect. At any rate, we may certainly claim as incontrovertible that, if there is a power which

* Wasmann, *Problem of Evolution*, p. 108.

has been put into living things which causes them to vary, and, still more, if it causes them so to vary that the result is a constant progress towards a definite goal, that power must have been infused and that course, with its goal, foreseen and foreordained by a Supreme intelligence. Plate thinks that we can know nothing of the "lawgiver." That is another question; it is something nowadays to have His existence admitted. Let us gather together some of the opinions and deductions which, as we have seen, are held to-day by various not insignificant men of science. There is the opinion that the changes which produce evolution originate from within, and not from without. There is the further opinion that these internal changes are the result of an inherent tendency, a power urging and guiding the organism along the path of progress. There is the view that this path of progress is pre-determined. There is the view that the changes which really count are sudden, considerable and discontinuous. Finally, there is the view that these sudden and considerable changes take place, not constantly, but at certain epochs in the history of a species: discontinuity in time as well as discontinuity in variation. Professor Poulton, who does not, as we gather from his writings, believe in any such internal force, admits or pleads—whichever is the right word to use—that the "idea of evolution under the compulsion of an internal force residing in the idioplasm is in essence but little removed from special creation."* It must be admitted that

* *Darwin and the "Origin,"* p. 20.

if we set together the various theories above enumerated, they form a coherent whole, and that that whole expresses in other words what might with considerable accuracy be described as a series of special creations, since the Lawgiver whose existence has to be postulated in order that the system may work is, whatever periphrase may be used, the God whom we reverence as the Creator of all things.

III: WEISMANN AND THE GERM-PLASM THEORY*

SINCE the appearance of Darwin's epoch-making works and more especially since the death of their author, various attempts have been made, at divers times, to take stock of the state of scientific opinion as to the doctrines and theories therein expounded.

To keep any accurate account of the passage of new theories more or less associated with this line of work would require, what every well-conducted business receives, an annual stock-taking; for as the seasons roll round and new flowers appear in our gardens, so fresh reconstructions and novel developments of the Darwinian hypotheses come to birth and, in at least some cases, imitate the flowers of the garden by blooming for a while and then disappearing. This is, of course, exactly what might have been expected from the launching into the world of a great and wide-embracing generalization, backed by the vast accumulation of facts which the patient work of its author had enabled him to collect. Of course, we all know that the essential features of the doctrine had been suggested by many others before Darwin arose; but those who most fully recognize this will also

* *The Evolution Theory*. By Dr. August Weismann. Translated by J. Arthur Thomson and Margaret N. Thomson. London: Edward Arnold. 1904.

most fully recognize that it was the learning of its greatest exponent, coupled with the fact that he made suggestions as to the methods of evolution, which will always associate the name of Darwin with the transformist theory. It is around the factors of evolution that controversy has raged and still rages most fiercely. For whilst most men of science freely accept the main hypothesis of evolution, biologists are divided into a variety of even bitterly hostile camps professing diametrically opposite views on almost every theory which attempts to explain how that process of evolution has taken place. Of course, it should again be borne in mind that it is not wonderful that there should be divergence of opinion on points in question. There is nothing intrinsically harmful in such divergence, perhaps even the contrary, for the clash of opinion has at least been successful in stimulating workers to search for and bring forward hosts of observations and facts which, without this stimulus, might have slumbered undiscovered for years, perhaps for ever. The regretable part of the business is that every new theory is put forward as if it were a divine revelation and not as if it were a mere hypothesis, and that its adherents at times may be found to treat their opponents with a scorn and contumely which would be painful if it were not amusing. Of course, it is perfectly true that the loudest-tongued in these frays are rather the camp-followers of science than the captains and generals in that army; yet even amongst these last there are not wanting those whose tongues would have done credit to

our army in Flanders as commemorated by Captain Shandy.

Now a hypothesis which collects together a number of otherwise isolated facts and makes a whole of them, is an achievement and a useful one, even if it subsequently turns out that the hypothesis is incorrect; for without working hypotheses science is arid, dead, non-existent. The mere observation of isolated facts, though a necessary preliminary to scientific generalizations, is a matter of comparatively small interest, and one which gives little if any stimulus to further work. It is the combination of these facts into a working hypothesis which gives stimulus and leads to further harvests of knowledge. All this one freely admits, yet without seeing why the newly erected hypothesis should be treated by its adherents as if it were the most sacred of doctrines. But this is actually what happens. A hypothesis is put forward and supported by a number of facts. Its originator collects around him a band of energetic workers and supporters who bring forward a number of further observations, all to the good of knowledge, capable of being interpreted in terms of the hypothesis and perhaps of other hypotheses also, if a little dialectical skill be employed. The new facts, thus interpreted, are held as being irrefutable proofs of the hypothesis which is now erected into a dogma. Severe scientific censures and cold contempt are the lot of those who cavil, so that the prime law of science, "Try all things," is almost forgotten, and those who would be critical, find it necessary to apologize for their audacity. What

a singular fact, for example, that one of the most prominent morphologists of the day should, in the preface to one of his works, "ask for a lenient judgment if in some pages of this work I have seemed to take up an unduly critical position with regard to views widely prevalent at the present time on some aspects of organic evolution!" Surely the more a scientific theory is criticized and sifted the better. But the dogma, for the time being, is sacred, so sacred that it is sometimes used to prove itself; that is to say, facts which might prove or disprove it are claimed as interpretable only in one way, because in any other way they would conflict with the dogma in question. Moreover, those who venture to doubt are discounted in some subtle manner. "The truth should now be frankly stated, that, as in the case of Cuvier and Owen, Professor Virchow's vast knowledge and range of thought have been somewhat neutralized by his excessive conservatism,"* says Professor Keane in explanation of the fact that Virchow, one of the giants of biological science, was not prepared to accept all the theories put forward by recent anthropologists. Haeckel assures us that "deep emotional disturbance, painful experience and exuberant hope" had "clouded the judgment" of Romanes, and prevented him from accepting the views of the author of *The Riddle of the Universe*. Or, finally, a man of science of the eminence of Lord Kelvin is told that he "speaks without authority" if he ventures on a philosophical statement, some of the underlying facts

* *Ethnology*, Cambridge, 1896, p. 144.

of which do not come within the domain of physics.

One would imagine that if there were any fields in which the flower of humility ought to be found flourishing those fields would be in the country of science, when one reflects that it is not merely of enormous difficulty to compass an irrefragable theory, but really an immense task to establish some of the facts on which vast superstructures of theory rest. Let us consider one example, an example which has been more often quoted than any other in evolutionary literature—the horse. The ancestry of the horse, in the first place, has been the commonplace of evolutionary text-books. One of the last of the many little manuals on this subject brought out by rival and enterprising firms of publishers tells us that "this great service, the affording of unquestionable proof of this momentous theory [of transformism] mankind owes to its trusty servant the horse." Yet, as has been pointed out time and again, there are various great difficulties still not cleared up in connexion with the much vaunted horse-pedigree, difficulties which render it quite absurd to assert that this affords "unquestionable" proof of the theory in whose favour it is brought forward. Sedgwick,* in his masterly work, states the case fully and fairly. "So far as the characters mentioned are concerned, we have here a very remarkable series of forms which at first sight appear to constitute a linear series with no cross connexions. Whether, however, they really do this is a difficult point to

* *Student's Text Book of Zoology*, vol. II, p. 600.

decide. There are flaws in the chain of evidence, which require careful and detailed consideration. For instance, the genus *Equus* appears in the Upper Siwalik beds, which have been ascribed to the Miocene age. It has, however, been maintained that these beds are really Lower Pliocene or even Upper Pliocene. It is clear that the decision of this question is of the utmost importance. If *Equus* really existed in the Upper Miocene, it was antecedent to some of its supposed ancestors. Again, in the series of equine forms, *Mesohippus*, *Miohippus*, *Desmathippus*, *Protohippus*, which are generally regarded as coming into the direct line of equine descent, Scott points out that each genus is, in some respect or other, less modernized than its predecessor. In other words it would appear that in the succession of North American forms the earlier genera show, in some points, closer resemblance to the modern *Equus* than to their immediate successors. It is possible that these difficulties and others of the same kind will be overcome with the growth of knowledge, but it is necessary to take note of them, for in the search after truth nothing is gained by ignoring such apparent discrepancies between theory and fact."

So much for the actual state of the case with regard to the pedigree of the horse. We have not yet wholly done with that animal. Apart from the evidence of his skeleton there is another line of argument which has constantly been based upon the occasional occurrence of stripings on the skin of the legs and withers, stripings which Darwin

thought were a reversion to the character of a very remote ancestor, the common ancestor, in fact, of all our present horses and asses ; an ancestor which was striped all over its body like a zebra. Of course, no one has ever seen this hypothetical ancestor, which is postulated to account for certain appearances, and may be a wholly imaginary creature. Darwin considered that the striped Kathiwar horse was a typical example of the primitive dun-coloured striped animal from which our domestic breeds have come ; and, as a matter of fact, such a Kathiwar horse is exhibited, as an example of the doctrine in question, in the National Museum of Zoology. Professor Ridgeway * has recently set himself to study again the horse problem, and has attacked this particular point with great acumen, indicating that a good deal depends upon whether the Kathiwar horses are indigenous and uncrossed, or at least uncrossed, for " if it should turn out that they are neither indigenous nor uncrossed, the argument founded on them by Darwin and succeeding writers will lose its validity."

And, after examining into the evidence, he concludes (p. 261) that these " dun-striped horses of Kathiwar are the result of crossing the upper Asiatic dun horses with Libyan blood." And, finally, he sums up the evidence which he has been able to collect upon this point as follows (p. 464) : " Darwin's view that the original ancestor of the Equidæ was a dun-coloured animal, striped all

* *The Origin and Influence of the Thoroughbred Horse*, Cambridge, 1905.

over, was based, not merely on the occurrence of stripes in horses, which we have just discussed, but on his belief that such stripes were common in dun-coloured horses, and that there was a tendency in horses to revert to dun colour. But it must be confessed that the facts do not warrant his conclusion. . . . It is clear that stripes are at least as often a concomitant of dark as of dun coloured. Moreover, if Darwin's hypothesis of a dun-coloured ancestor with stripes is sound, dark colours such as bay and brown must be of more recent origin, and accordingly there ought to be a great readiness on the part of progeny of a light-coloured animal when mated with a dark to revert to the light. But Prof. Ewart's zebra stallion has never been able to stamp his own peculiar pattern or his own colours on his hybrid offspring. The ground colour has been determined by the dams of the hybrid."

We have dwelt upon these points because it seems to us that the very great difficulties which quite obviously exist in ascertaining the exact state of the case in connexion with so many of the underlying facts—for many other matters as to which there is also grave doubt could be quoted—to say nothing of the difficulty of interpreting them, should indicate the necessity for great caution in accepting or claiming as proved, in the present state of knowledge, any but a few of the modern biological doctrines.

Amongst these doctrines and theories the views enunciated by Weismann, by Mendel and by de Vries within recent years have attracted a very

great amount of attention, and have profoundly influenced and are influencing the trend of scientific thought. With Weismann's theories I propose to deal in the remainder of this article.

Weismann has been before the world as a scientific writer for a number of years. He became familiar to workers when his *Studies in the Theory of Descent* were translated by Professor Raphael Meldola and given to the public more than thirty years ago.

But it was not until the remarkable series of essays, subsequently translated and brought out in a volume by the Clarendon Press, under the editorship of Professor Poulton, began to appear, that he attracted that amount of attention which has since been his lot. One need only mention his remarkable work, *The Germ-Plasm*, published in its English dress by Walter Scott, to remind ourselves that the views put forward in the volumes with which we are now dealing are not there enunciated for the first time. But these two great volumes contain the summing-up of his life-work, the pith and marrow of all the books which have preceded them.

No one who carefully reads the chapter "On the Mechanical Conception of Nature" in the *Studies in the Theory of Descent* would imagine that Professor Weismann belonged to the category of blank materialists. He himself says (vol. ii., p. 712)

"The consciousness that behind that mechanism of the universe, which is alone comprehensible to us, there still lies an incomprehensible teleological

universal cause necessitates quite a different conception of the universe—a conception absolutely opposed to that of the materialists."

And further (p. 716):

"I believe that I have shown that the theory of selection by no means leads, as is always assumed, to the denial of a teleological universal cause and to materialism . . . Mechanism and teleology do not exclude one another; they are rather in mutual agreement. Without teleology there would be no mechanism, but only a confusion of crude forces; and without mechanism there would be no teleology, for how could the latter effect its purpose?"

Yet, on the other hand, it must be admitted that his views as to his "teleological universal cause" are nebulous in the extreme; perhaps even more nebulous in his later than in his earlier work. He quotes with approval and even adopts as his own the words of Erasmus Darwin: "All that happens in the world depends on the forces that prevail in it, and results according to law; but where those forces and their substratum [matter] come from we know not, and here we have room for faith." Yet he concludes that "at no time have organisms been called forth out of nothing by the mighty word of a Creator, but have been produced at all times by the co-operation of the existing forces of nature"—the old phrase which gets us no farther forward, since it tells us nothing as to how nature, itself only a

personification, attained the powers by which these mighty works have been done.

It would be a hopeless task to attempt to enter into any account of all the points dealt with in these two large volumes. Nor is that task necessary, for large portions of them are occupied with an account—a most interesting account—of the main lines of the transformist theory, lines with which all educated persons are familiar. It will serve our purpose if we follow out one line of argument, that touching the influence of the environment on the individual and his progeny; for in following out that line we shall be brought in contact with the main features of Weismann's teaching, features with which no person interested in the progress of science at the present day can afford not to be acquainted.

Jean Baptiste de Lamarck was born in 1744 in a village in Picardy. He made a considerable reputation by his *Flora of France*, and merits a niche in the temple of science as the founder of the category of "vertebrates." For neither of these reasons, however, is his name now familiar, but for the fact that he was the author of the *Philosophie Zoologique*, a work which, oddly enough, appeared in 1809, the very year in which Charles Darwin saw the light. In this work—one of the pre-Darwinian expositions of the doctrine of transformation—Lamarck taught that any great alteration in the conditions of life, if sufficiently prolonged, would cause a change in the needs of living things affected by them, and that this in time would cause them to adopt new habits. But

new habits can scarcely be formed without some change taking place in the bodily organization, for they must give greater exercise to some parts and less to others than the earlier habits. The parts now more freely used, perhaps indeed now used for the first time, will tend to become more fully developed and will improve, whilst those which have fallen into desuetude will tend to atrophy and degenerate. To these theses there will be little opposition. No one will deny that a man's muscles may be enlarged and improved by long-continued and well-regulated exercise. But it is another thing to argue, as some do, that new organs may arise in response to new needs. And it is also another thing to admit that the improvement in the old organs may be transmitted or that the newly-acquired organs may be transmitted to a younger generation. Lamarck died poor, blind and little regarded by his contemporaries: yet his influence is still strong, and the neo-Lamarckians are an important wing of the biological army. A single example will explain the meaning of Lamarck's views and the distinction between them and the views of Darwin. The giraffe is provided with an extraordinarily long neck and very tall forelegs. These he acquired, according to the Lamarckian view, by constantly stretching after the foliage of trees, on which he feeds, and by ever reaching after higher and yet higher boughs. According to the Darwinian view certain giraffes were by reason of causes inherent in the embryo provided with somewhat longer necks than their fellows. In time of stress these giraffes could get

food where others could not. Hence they survived, and their progeny, also long-necked, gave rise to animals with still further development in the same direction. This contrast will serve as an example of the difference in interpretation of the facts of the two men. The neo-Lamarckians take up the position that new habits may produce new organs. For example, Cunningham* argues that Darwin's doctrine of selection can "never get over the difficulty of the origin of entirely new characters," and proceeds :

"It may be said that the necks of the giraffe's ancestors were of different lengths, and the selection of the longest produced the striking length of neck we now see. But how can it be said that the horns of ruminants arose ? No other animals have ever been stated to possess the two little symmetrical excrescences on their frontal bones as an occasional variation ; what, then, caused such excrescences to appear in the ancestors of horned ruminants ? Butting with the forehead would produce them, and no other cause can be suggested which would."

Darwin saw that variations occurred ; he saw that they might be inherited, and he postulated the existence of certain selective forces, natural and sexual, which could pick out the beneficial variations and fix them, just as a given variation is picked out and fixed by the breeder of domestic animals. Now with respect to the facts that variation takes place and that there is such a thing as

* *Organic Evolution*, by Eimer. Translated by Cunningham. Translator's preface, p. xx.

heredity there can be no doubt, for it is a matter of common notoriety that no descendant is exactly like his progenitors though more or less resembling them.

With the question of natural selection we cannot deal in this article, but the real point is that before your variation can be selected, naturally, artificially or sexually, it must originate, and the question is how it originates. Lamarck believed in the influence of the environment, and to a less extent so did Darwin ; but in sharp contradistinction to these are at least the earlier views of Weismann with regard to the heredity of acquired conditions.

In dealing with this part of the subject it will first be necessary to distinguish mutilations from other kinds of acquired conditions. When one comes to think of the number of artificial deformities which have been regularly produced by different races for centuries : circumcisions, breakings of teeth, piercings of ears, nose and lips ; and when one further reflects that there are no instances of the transmission of such deformities, still less of the formation of a permanently deformed race of individuals, it is rather difficult to understand how the belief in the possible heredity of mutilations has survived so long. It must be set beside that other belief in maternal impressions, so old, so often exploded, yet still so firmly held by many. It is not possible here to go into the evidence in connexion with this question of mutilations. Those who desire to see the subject fully treated may be referred to the last essay in the

Clarendon Press collection of Weismann's papers. Here it need only be said that belief in the transmissibility of mutilations has now disappeared from scientific minds.

As regards the inheritance of other acquired conditions, Weismann was at first supposed to deny that the environment could produce any effect upon the included germs and therefore upon the individuals into which they might develop. We say "supposed," for this was generally taken to be his meaning, though, in the work now under consideration, he states that this view was a misunderstanding of his position, and complains (ii. 195) that he has "been frequently and persistently credited with maintaining that the germ-plasm is invariable." In any case, as will shortly be seen, he only allows that the environment can operate in a very roundabout manner upon the germ-plasm. But what is this germ-plasm, and what, according to Weismann, are its characters? In order to explain this one must first consider the condition of a unicellular organism. Here we have an individual in which all functions, respiration, digestion and the like, take place in the same cell. When multiplication is to occur, the cell divides into two, each becoming a new and separate individual, neither of which can be spoken of as mother or as child. It is clear that such an individual can be killed by deprivation of moisture or by other means; but, given suitable conditions, there is no reason why such a form should ever die; it is potentially immortal.

The first stage in the direction of complexity

is to pass from the unicellular to the multicellular condition. A differentiation in function then takes place, certain cells being capable of one duty but incapable of another, until we come to the condition of complexity met with in the mammal, for example, where we have nerve cells, muscle cells, liver cells and the like, all derived from the single-celled fertilized ovum, and all capable of performing their own functions and no others. Now, according to Weismann, the first stage in this process of differentiation is the cutting off of some of the original germ-substance to form the germ-substance of future generations. This substance, thus cut off, is as much derived from the first germ as the two unicellular organisms were developed from one. It is consequently, like these organisms, potentially immortal. The rest of the germ, which goes to make the body or soma, is mortal; it is the home, the protector and the nourisher of the immortal germ-plasm. The soma is the visible part; the germ-plasm is invisible except under the microscope; but it is the invisible part which is handed on, not the visible. Hence, according to this view of things, the germ-plasm in an individual is the same as that which was in his progenitors, and in theirs back to the *n*th generation. Consequently the individual really is his ancestors, and, therefore, naturally would resemble them.

This is the first and most fundamental point in Weismann's theory. Now for the next. Is this germinal substance (which he considers to be the chromatin filaments of the nucleus of the germinal

cell) a simple substance, or has it an architecture, so to speak, of its own? Simple in chemical composition it is not, for dead nuclein, which is, of course, the only kind which can be examined, has a complicated chemical formula. But is it otherwise simple or indifferent? To put the matter plainly, if a little crudely, suppose we could divide the chromatin up into its ultimate units—not chemical but vital—always supposing that it consists of such units, would all these units be like to one another and capable, in case of necessity, of replacing one another? Or, on the other hand, would each have its own characteristics, as the cells of the human body have each their own characteristics and functions? Such is the guise under which the old question of preformation or epigenesis now presents itself to us. To the preformationists—or evolutionists, as they were then called—of the eighteenth century, unacquainted with the knowledge which the microscope has given us of the germ cells, the germ was almost a miniature—of course an extremely minute miniature—of the adult form, and its development was merely an unfolding of already existing parts. Bonnet, who was the protagonist of this theory, did not hold that his miniature model was exactly like the perfect animal, but taught that it consisted of "elementary parts" only, which he thought of as a net, whose meshes were filled up, during development, and by means of nutrition, with an infinite number of other parts.

On the other hand the supporters of epigenesis the first of whom was K. von Wolff, held that there

was no miniature model in the germ. According to this school there arose in the simple substance of the germ, from the formative powers inherent in it, a long series of stages of development, each one more complex than that which preceded it, until the perfect form was arrived at. Weismann and Hertwig may be looked upon as the exponents of the modern forms of these two views. The former believes that development is the becoming visible of complexity previously hidden from us; the latter that it is a new formation of complexity.

"I assume [says the former] that the germ-plasm consists of a large number of different living parts, each of which stands in a definite relation to particular cells or kinds of cells in the organism to be developed, that is, they are 'primary constituents' in the sense that their co-operation in the production of a particular part of the organism is indispensable, the part being *determined*, both as to its existence and its nature by the predestined particles of the germ-plasm." Vol. i. 355.

Hertwig, on the contrary, holds that

"the embryological development of an organism is no mosaic work. The parts of an organism develop in relation to each other, the development of a part depending upon the development of the whole."*

It will be seen that these two views are diametrically opposed to one another.

Let us now follow Weismann's theory a little

* *The Biological Problem of To-Day.* Translated by Chalmers Mitchell. Heinemann, 1896.

more closely. The germ-plasm of a given cell is, he teaches, made up of a number of "ids," each of which is the complex of primary constituents necessary to the production of a complete individual. A compound of several of these is an "idant." The "id" is made up of "determinants," and there is one determinant for each portion of the body which is capable of independent variation. Some of these must belong to very minute areas.

"Thus, for instance, in many human families there occurs a small pit, hardly as large as the head of a pin, in the skin of the ear, whose transmission I have observed from the grandmother to the son and to several grandchildren. In such a case there must be a minute something in the germ-plasm, not present in that of other human beings, which causes the origin, in the course of development or this little abnormality in the skin."—Vol. i. 355.

The "minute something" is the determinant for the part. It is obvious from this that there must be an enormous number of these determinants in the simplest form of body. But under further developments of Weismann's system the multiplicity becomes something almost unthinkable. For it is clear that if we are to have a separate determinant for every part of the body capable of independent variation, and if—as we know is the case—the caterpillar and the butterfly, which comes from it, are both of them capable of independent variation, then the caterpillar must carry in its body the determinants which are

subsequently to form the butterfly. Moreover, both sets must have existed side by side in the egg. Then there are the reserve determinants which will be mentioned in connexion with the question of regeneration. And, finally, we even hear of "highly developed musical determinants, which we must assume in the case of musical genius" (Vol. ii. 149).

Smaller even than the determinants are the "biophors," which are the vital units of the germ, and may be looked upon as separate living things, so to speak, existing in that microcosm the germ-cell; units "which feed and reproduce, which assimilate and which bear a charm against the assimilating power of the surrounding protoplasm" (i. 401).

In some shape or another such vital units have been thought of by many scientific writers, since we have the "physiological units" of Herbert Spencer, the "gemmules" of Darwin, the "idioblasts" of Hertwig, the "micellæ" of Nägeli, and the "pangenes" of de Vries. These vital units are the bearers of the characters of cells in the adult organism.

Perhaps it is well to pause at this moment, and to remind the reader that all these things just named and described are pure assumptions, not one of them being demonstrable by the microscope, unless indeed an "idant" may be the same as a single thread of chromatin, which is an unproved and probably unprovable assumption. It is necessary to insist upon this because, when reading the book, one is apt to be so much carried

away by the writer's earnestness and thorough faith in his own views, as to forget that the things of which he is treating are pure creatures of his imagination, which may possibly have an existence, but, on the other hand, quite as likely may not. Now let us turn to some of the difficulties which have been brought forward. Let us see how Weismann answers them. During this part of our examination we shall be reminded of the writer who said, "Give me my one impossible situation, and I will construct you a story." For Weismann seems to say: "Give me my determinants, and I will explain anything you can ask me." First, it is inquired how galls can be accounted for, since there can be no such things as gall-determinants. "Of course there are not," replies Weismann, but "the specific nature of the different kinds of plant-cells, predetermined by their determinants, is such that, through the abnormal influences exercised upon them by the larvæ, they are compelled to a special reaction which results in the formation of galls" (i. 385). Our writer will hear of no vital phenomena in the cell or the plant, but surely in the explanation given above—a kind of biological parallelogram of forces—we are not very far from something like the famous *virtus dormitiva*.

Then, in the second place, he is asked how he accounts for the remarkable phenomena connected with the shaking apart of the cells which make up the eight-cell stage of the development of *Amphioxus*. Here each cell, which under ordinary circumstances would have formed one-eighth of

an *Amphioxus*, forms an entire and undivided *Amphioxus*. And his reply is, "it is only necessary to assume that the segmentation-cells, which develop in the isolated condition as if they were intact eggs, still contain the complete germ-plasm, and that the differential segregation into groups of determinants with dissimilar heredity tendencies takes place later" (Vol. i. 406).

Again, he is called upon to deal with the great question of regeneration. The fresh-water *hydra* can be cut up into almost any number of pieces, and out of each one of them a new complete *hydra* may arise. The same may be done with a little moss known as *Funaria hygrometrica*, from every minute fragment of which a new plant will arise. Think how many willow-trees can be grown from a single willow by means of cuttings! Passing to worms, *Lumbriculus* has been cut up into twenty-six pieces each two millimetres in length, and most of them formed new complete animals. A much more highly organized animal, the water-newt or triton, can regrow its limbs. Spallanzani six times cut the legs off a single newt, and six times the animal grew those limbs again. Weismann admits that these cases certainly do not afford any special evidence of its validity," *i.e.*, the validity of his theory, "as an interpretation" (xx. ii. 7), but he has his reply, for speaking of the *hydra*, he says:

"The animal's high regenerative capacity must, therefore, depend on the fact that certain cells of the ectoderm are equipped with the complete

determinant-complex of the ectoderm, in the form of an inactive accessory idioplasm, which is excited to regenerative activity by the stimulus of wounding, and that, in the same way, the cells of the endoderm are equipped with the whole determinant-complex of the endoderm."—Vol. ii. 5.

Let us again pause and consider the enormous complexity of the cellular mechanism postulated by this theory. Each cell, or, at least, a very large number of the cells, in an organism like *hydra*, must contain not merely the substances required for the carrying on of its own activities, but also the highly complex determinants necessary for the rebuilding of the body, should an accident occur and the cell in question be called upon to take up that duty. Remember also that this is an accident which may not happen to more than one out of a million cells so provided. Surely this is a very wasteful expenditure of the highly organized germ-plasms and its determinants. Of course, the complexity of the theory is not necessarily an argument against it. It is not impossible for the tiny sphere of the germ-cell to be packed with a multitude of independent fragments. Nägeli calculated that a "moneron" .6mm. in diameter might contain a 100 billions of vital particles, if its dry substance be taken as ten per cent. of the whole. Professor M'Kendrick says:

"Small as the reproductive body formed by the fusion of the male and female elements is, it is still large enough to contain *millions* of organic

molecules having a complexity of structure as great as that of a molecule of albumen."

Well has it been observed that

" the physicists report that the image of a *Great Eastern* filled with framework as intricate as that of the daintiest watch does not exaggerate the possibilities of molecular complexity in a spermatozoon, whose actual size may be less than the smallest dot on the watch's face."

But it is at least fair to remark that the theory is a tolerably complex one to be built up entirely upon a system of " vital units," which no one has ever seen or can ever demonstrate. The author of the theory frankly admits all this, but contends that his assumptions are justified by the facts of nature. Well, let us see what another eminent scientific man has to say on that head. Professor Hertwig of Berlin is a man whose name is known wherever biologists exist. He has no *odium theologicum*, nor is there any other reason why he should regard the facts as teaching a different lesson, but the reason that Weismann's assumptions are not the kind of assumptions which appeal to him. Let us hear what he says of the views which we have been explaining :

" When, to satisfy our craving for causality, biologists transform the visible complexity of the adult organism into a latent complexity of the germ, and try to express this by imaginary tokens, by minute and complicated particles coherent into a system, they are making a phantasmal

image which, indeed, apparently may satisfy the craving for causality (to satisfy which it was invented), but which eludes the control of concrete thought, by dealing with a complexity that is latent and perhaps only imaginary. Thus, craftily, they prepare for our craving after causality a slumbrous pillow, in the manner of the philosophers who would refer the creation of the world to a supernatural principle. But their pillow of sleep is dangerous for biological research; he who builds such castles in the air easily mistakes his imaginary bricks, invented to explain the complexity, for real stones. He entangles himself in the cobwebs of his own thoughts, which seem to him so logical, that finally he trusts the labour of his mind more than nature herself." *

Poor Weismann! this was "the most unkindest cut of all," that he should be reckoned as no better than one of those who believe in a Divine Creator.

But we have not yet done with our "vital units" and their performances. They not merely exist, side by side, in that microcosm the germ-cell, says their inventor, but they war ceaselessly with one another. This struggle between the vital units is the process of "germinal selection" which Weismann postulates in order to account for the occurrence of variations and to explain how the environment can act on the germ. There are, he says, marked fluctuations in the nutritive stream supplied to the germ-plasm as it lies in the body; fluctuations which may be, and, in many cases,

* *The Biological Problem of To-day*, p. 11.

doubtless are, the result of changes in the environment. Such changes in the nutritive stream may affect some of these determinants favourably and some unfavourably. Thus in that internecine strife in which they are engaged, some will be assisted to gain the mastery, whilst others have impediments placed in their way. Such very briefly is the theory of germinal selection; and any one who considers carefully what it means will see that, through it, Weismann once more comes back to that influence of external conditions upon the germ which he set out, if not to deny, at least to minimize. For if the vital units make up the germ, and the vital units can be affected by the nutritive stream, and this again may be altered by external conditions, is it not clear that external conditions alter the character of the germ, which is precisely what Lamarck and Darwin claimed? Let us look at one example of this. *Polyommatus phlæas*, the "fire-butterfly," presents two forms, a northern and a southern, with different colourings. Now the colouring of the southern form can be induced in the northern, by the action of a higher temperature, showing

"that the direct influence of higher temperature affects the quality of the nutritive fluids in the germ-plasm, and thereby at the same time the determinants of one or more kinds of wing-scales are caused to vary in all the ids in the same direction, in such a fashion that they give rise to black scales instead of the former red-gold ones. It is thus certain that there are external influences

which cause particular determinants to vary in a particular manner."—ii. 137.

It will now begin to be clear to the reader that it is possible to explain anything which may be discovered in nature in terms of the "vital unit" theory. On the one hand "the phyletic modification of the limbs in the ungulates has taken place with striking uniformity in the fore and hind extremities: no animal has ever been one-hoofed in front and two-hoofed behind" (vol. ii. 189). Clearly we have to do with "germ-plasmic correlations; and we have assumed from the very first that the different determinants and groups of determinants do indeed stand in definite and close relationship to one another" (vol. ii. 189). On the other hand, we find the kangaroo with remarkably large and strong hind-limbs and equally remarkably small and weak fore-limbs—that is to say, with a striking want of uniformity in his fore and hind extremities. "This, it seems to me, is only possible when amphimixis"—of which we have yet to treat—"brings about in one individual a favourable coincidence of the chance germinal variations of the determinants of the fore and hind-limbs" (vol. ii. 197). Finally, as a crowning example of what may be done in the way of explaining facts, *lumbriculus*, of which mention has already been made, can regenerate itself if cut into a number of segments transversely, but cannot regenerate itself if it is divided longitudinally.

"The reason must lie in the fact that the primary constituents for this kind of regeneration

are wanting, and they are so because a longitudinal splitting of this cylindrical and relatively thin animal never occurs under natural conditions, and thus could not be provided against by nature."

Here again we seem to be within measurable distance of our old friend the *virtus dormitiva*.

Mention has just been made of the word *amphimixis*, a state of affairs in which Weismann finds another cause of variation. It is well known that whilst most forms above the rank of unicellular organisms multiply bi-sexually, parthenogenesis occurs in other forms, and, it may be added, in a rather wide range of forms.

In the ova of both forms certain changes take place before development or even fertilization—in the cases where bi-sexual development or amphimixis is the rule—can commence. These changes consist in the extrusion of part of the nuclear material in the form of what are known as "polar bodies." Two of these are cast out in cases of bi-sexual, one in parthenogenetic development. Now, according to Weismann, this process relieves the ovum of a certain number of particles of ancestral germ-plasm, chance determining which may be taken and which left. Thus in one descendant the germ-plasmata of a, e, i, o, u ancestors may be got rid of, in another those of v, w, x, y, z. The result will be that the two descendants will vary in accordance with the kind of germ-plasms which they retain or lose. Hence amphimixis, or bi-sexual development, is, according to his reading of the facts, a mechanism for causing variation.

According to others, as Haycraft, it is a mechanism for keeping the race constant—an absolutely opposite conclusion. Karl Pearson, from a biometrical study of the results of the two kinds of development, says, "Variability is not a product of bi-parental inheritance. . . . Whatever be the physiological function of sex in evolution, it is not the production of greater variability." And Archdall Reid concludes :

"Though nearly all biologists have supposed that progressive variation, and therefore evolution, is largely due to bi-parental reproduction, there is, in fact, in the whole range of biological literature not one iota of evidence which supports that view. Men, as in so many instances, have accepted a dogma without proof and have held it without inquiry."

Here then again we come to a direct and fundamental difference of opinion amongst men of science as to the interpretation of one of the best-known and most remarkable phenomena of life, that of bisexual development. In the face of this contradiction, what is one to think of theories which are built upon so uncertain a foundation ? That the question should be discussed ; that any number of facts should be brought together ; that provisional hypotheses should be set up with a view to seeing how facts tally with them—all this is well. But that such hypotheses should be erected into dogmata and belief in them demanded, is to ask more than one is prepared to concede.

Weismann's "vital units" live their own lives,

so to speak. But granting this to be true we are still confronted with the question : What makes each of them live ? What sets agoing and keeps agoing the processes, complicated and potent, of which these little units are the theatre ? To this question Weismann has a clear negative answer. It is not a vital process of any kind. If one seeks for a further explanation and asks why vital influences are out of court, it is true that one obtains no very certain response. In the last analysis it would seem to be argued that there is no such thing as a vital force because it cannot be seen or demonstrated, a form of argument which, if one agreed with it, might be turned with considerable force against the doctrine of the "vital units" itself, and indeed against the whole of the germ-plasm theory.

But if there is no such thing as vital force, what does this work ? Is it a chemical, or a physical, or a chemicophysical operation ? It is quite clear that no explanation of this kind will suffice.

" Thus from all sides we are forced to the conclusion that the germ-substance on the whole owes its marvellous power of development not only to its chemico-physical constitution, whether that be eminently simple or marvellously complex, but to the fact that it consists of many and different kinds of ' primary constituents ' (*Anlagen*), that is groups of vital units equipped with the forces of life, and capable of remaining latent in a passive state, until they are affected by a liberating stimulus and on this account able to interpose successively in development."—Vol. i. 402.

Again we hear of "internal forces"; of "vital affinities" (vol. i. 347); and of mysterious "stimuli."

"That double determinants, male and female for the differently formed parts of the two sexes, must be assumed to exist in the germ-plasm has already been said, and we have to suppose that the same stimulus—usually unknown to us—which incites the determinants of the primary sexual characters to activity also liberates those of the secondary characters."—Vol. i. 389.

Some of these may be external stimuli, but there are also "intra-organismal stimuli, that is, the influences exerted in a mysterious manner by other parts of the animal on parts which are in process of regeneration" (vol. ii. 24). Finally we read of "constellations of energy, co-ordinations of matter and the energies immanent therein" (vol. i. 403).

But it is clear that none of these things are, in their describer's mind, at all like a "vital force," for this he denies and brings arguments to show that it is not necessary "to fall back upon a belief in a *spiritus rector* in the organism" (ii. 19).

Lastly, has the author anything to tell us as to how these vital units first came into existence? Yes, he has essayed a reply to this question also.

As we have already concluded that "at no time have organisms been called forth out of nothing by the mighty word of a Creator" (i. 6)—though nothing is said as to the possibility of a Creator having formed living organisms from pre-existent

matter—there are "only two possibilities: either they have been borne to our earth from outside, from somewhere else in the universe [which would, of course, only shift the question of their origin one stage further off], or they have originated upon our earth itself through what is called 'spontaneous generation'—*generatio spontanea*" (ii. 364). Now, as we all know, every effort to prove the existence of spontaneous generation has so far failed. It is true, all that this amounts to is that no experiment has ever yet succeeded in showing that spontaneous generation takes place, and there are those who argue that some experiment may yet turn out to be successful. Well, science is based on observed facts; and this at least may be said that all the facts so far observed, and all the observations of Pasteur and the most acute experimentalists who have ever yet lived, are totally opposed to the theory of spontaneous generation. Others, again, have held, like Huxley, that if we could "look beyond the abyss of geologically recorded time," we might be witnesses "of the evolution of living protoplasm from not-living matter." Weismann agrees with those who deny the possibility of the introduction of living matter on a meteorite from some other sphere, and hence for him spontaneous generation is a "logical necessity" (ii. 366). He admits that up to now "all attempts to discover the conditions [under which spontaneous generation might occur] have been futile." Moreover, he does not believe that such experiments can ever be successful, "not because the conditions must be so peculiar in nature that we

cannot reproduce them, but above all because we should not be able to perceive the results of a successful experiment" (ii. 367). Here again, we find ourselves in a region where observed fact is impossible. Huxley desired us to cast our gaze upon a period which no man ever saw and of which no man can know anything; Weismann tells us that if there had been a man there, and if he had been provided with the most complete scientific armamentarium, he could not have seen what was taking place. And this, he continues, "I shall be able to prove convincingly without difficulty." What is his proof? Well, if one admits the existence of vital units living side by side in the microcosm of the germ, it is only one step further to imagine them carrying on independent existences outside the germ as detached organisms. Clearly then the first organisms were biophors, and, as we cannot see a biophor, it is obvious that when they first appeared, even if the observer had been there and had been searching for them, his search must have been in vain. Indeed it would be hard to prove that they are not being produced all around us every day at this very moment.

It may be well to quote the actual words of the writer on this point.

"In regard to them [the biophors] alone is the possibility of origin through purely chemico-physical causes, without the co-operation of life already existing, admissible. It is only in regard to them that spontaneous generation is not inconceivable. We must, therefore, assume that, at some

time or other in the history of the earth, the conditions necessary to the development of these little invisible particles must have existed, and that the whole subsequent development of the organic world must have depended upon an aggregation of these biophors into larger complexes, and upon their differentiation within these complexes. We shall never be able, then, directly to observe spontaneous generation, for the simple reason that the smallest and lowest living particles, which could arise through it, the Biophoridæ, are so extremely far below the limits of visibility that there is no hope of our ever being able to perceive them even if we should succeed in producing them by spontaneous generation."—ii., 369.

In his amusing excursus on the "awful German language." Mark Twain declares that the sentences in that language are too much given to parenthesis; there is the parenthesis, the re-parenthesis, the re-re-parenthesis and the all-embracing King parenthesis. So in this theory we have the assumption, the re-assumption, the re-re-assumption and the all-embracing King-assumption. It is assumed that the substance of the germ-cell is not simple but complex; it is assumed that this complex body is made up of determinants for different parts of the body; it is asssumed that these again are built up of vital units each living its own life, struggling with its neighbours, influenced by the nutritive stream by which it is bathed; and, finally, by an all-embracing King assumption, these unseen, unprovable vital units

are erected into a new family of living beings, the "Biophoridæ," and we are told they were spontaneously generated, and that no man can prove the contrary, for they are, and must always be, invisible. Surely the spinning of theories can go no further than this.

Even its author at times allows himself to doubt; for in one place he says—and with these wise words we must leave the subject of this book:

"While dead albumen is certainly nearly allied to living matter, it is precisely life that it lacks; and as yet we do not know what kinds of chemical difference prevail between the dead proteid and the living; indeed, we must honestly confess that it is a mere assumption when we take for granted that there are only chemicophysical differences between the two. It cannot be proved, in the meantime, that there is not another unknown power in the living protoplasm, a "vitalistic principle," a "life-force" on the activity of which these specific phenomena of life, and particularly the continually repeated alternation of disruption and reconstruction of the living substance, dissimilation and assimilation, growth and multiplication depend" (vol. ii. 369).

IV: DE VRIES AND THE THEORY OF MUTATIONS*

THAT Darwin invented the theory of transformation or the derivation of one species from another is an impression apparently indelibly engraved on the minds of the half-educated many who pride themselves on their acquaintance with science. Of course neither Darwin himself nor any of his scientific followers have ever made this claim on his behalf, for he and they were perfectly well aware that in one way or another a theory of transformation had been put forward from time to time from at least the days of St. Augustine.

The title page of Darwin's epoch-making work generally known under the name of *The Origin of Species* is really, as already pointed out in previous articles, *The Origin of Species by Natural Selection; or, The Preservation of Favoured Races in the Struggle for Life.* It was the method not the fact of evolution which Darwin chiefly endeavoured to demonstrate; and around the question of natural selection controversy has always raged and still continues to rage. I have already dealt with

* *Species and Varieties* Their Origin by Mutation. By Hugo de Vries. Chicago. 1905.
Evolution and Adaptation. By Thomas Hunt Morgan. New York. 1903.
Materials for the Study of Variation. By William Bateson. London: Macmillan. 1894.

that question in a previous article, but there are some other points to be dealt with, notably those of Mutations. And in this present article I propose to deal with the views of the distinguished Amsterdam botanist whose name is associated with that theory.

According to Darwin natural selection acted upon spontaneous slight variations constantly occurring in all species of plants and animals. "Natural selection," he says, "acts only by the preservation and accumulation of small inherited modifications." This view as to the accumulation of small modifications was acutely criticized on its first appearance before the world by Mivart, who stated that natural selection utterly fails to account for the conservation and development of the minute and rudimentary beginnings, the slight and insignificant commencements of structures, however useful these structures may afterwards become."* Moreover, he argued that such small peculiarities might, before they had sufficiently developed to be of real advantage to their possessor, be an actual injury as affording further and useless living material requiring nourishment. And in this connexion he enters into a long consideration of Darwin's theory as exemplified in the neck of the giraffe, a discussion which will be quite familiar to all students of the evolutionary theory.† But further he argued (p. 109) that "there are difficulties in the way or accounting for such origination by the sole action of

* *Genesis of Species*, p. 26.

† See p. 68 for an account of this matter.

modifications which are insignificant and minute, whether fortuitous or not." And he proceeds: "Arguments may yet be advanced in favour of the view that new species have from time to time manifested themselves with . . . .suddenness, by modifications appearing at once (as great in degree as are those which separate *Hipparion* from *Equus*), the species remaining stable in the intervals of such modifications: by 'stable' being meant that their variations only extend for a certain degree in various directions, like oscillations in a stable equilibrium. This is the conception of Mr. Galton,* who compares the development of species with a many-faceted spheroid tumbling over from one facet, or stable equilibrium, to another."

Whether the change takes place rapidly or gradually from one facet to another, the result is the production of a form widely—comparatively speaking widely—differing from the original individual. Is this the production of a new species? In answering that question we are naturally confronted with the long-standing inquiry as to what after all is meant by a species. Of course in nature there are no such things as species, for species are only categories invented by men for the purpose of aiding classification. But looked at from that point of view it is by no means easy—is it even possible?—to say where a species ends and a variety begins. It is quite easy to separate a flowering plant from a cryptogam; so also is it easy to distinguish a member of the rose family from one belonging to that of the willows. But when we

* *Hereditary Genius: An Inquiry into its Laws, etc.*

get a stage or two further and try to separate the various members of the families Rubus or Salix from one another, we find ourselves in what seems to be inextricable confusion, unless perchance one is gifted with the peculiar kind of mind belonging to the person sometimes unkindly called a "species-monger." At any rate botanists, at least, seem to have arrived at the opinion that amongst the groups commonly called "varieties" there are groups which resemble in many ways the groups of species in other forms, since they breed true to their kind, even under changed conditions. They have been recognized as "smaller species" by a number of botanists.* "Elementary species" is the name given by de Vries, and he points out that "we must recognize two sorts of species. The systematic species are the practical units of the systematists and florists, and all friends of wild nature should do their utmost to preserve them as Linnæus has proposed them. These units, however, are not really existing entities; they have as little claim to be regarded as such as the genera and families have. The real units are the elementary species" (p. 12). And again: "Linnæus himself knew that in some cases all subdivisions of a species are of equal rank, together constituting the group called species. No one of them outranks the others; it is not a species with varieties, but a group consisting only of varieties. A closer inquiry into the cases treated in this manner by the great master of systematic science shows that here his varieties were exactly

* Morgan, p. 33.

what we now call elementary species" (p. 13). Those desirous of examples of what is here described may be referred to de Vries' account of the different varieties of elementary species of Viola.* It is possible, so it has been argued, that these "smaller species," or "elementary species," or "constant varieties" are "incipient Linnæan species, which, by further variations of the same, or of other sorts, may end by giving rise to true species. A genus composed of several species might be formed in this way, and then, if each species were again broken up into a number of new groups, each such group would now be recognized as a genus, and the group of genera would form a family, etc. The process continuing, a whole class or order, or even phylum, might be the result of this process that began in a single species."†

At any rate we can see for ourselves that, as Bateson puts it, "the forms of living things are various and, on the whole, are discontinuous or specific" (p. 3); and what we have now to discuss is how, assuming transformism to be the explanation of the state of nature as we see it, this discontinuity came about. Was slight change added to slight change until at last a new "elementary species" stood revealed? or did each "elementary species" suddenly make its appearance as a definite and complete entity? How, in other words, was differentiation introduced into the series of which the species we are now considering may be regarded as, for the moment, the last term? All that we know, as Bateson points out,

* pp. 38 et seq. † Morgan, p. 85.

is this last term. "By the postulate of common descent," he continues, "we take it that the first term differed widely from the last, which nevertheless is its lineal descendant; how then was the transition from the first term to the last term effected? If the whole series were before us, should we find that this transition had been brought about by very minute and insensible differences between successive terms in the series? or should we find distinct and palpable gaps in the series? In proportion as the transition from term to term is minimal and imperceptible, we may speak of the series as being continuous; while in proportion as there appear in it lacunæ, filled by no transitional form, we may describe it as discontinuous" (p. 15). Continuous or discontinuous? That is the question around which the present article turns.

We have already seen how Darwin answered the question, and that Mivart doubted the correctness of his conclusion. De Vries' work, with which we are here concerned, is devoted to the maintenance of the thesis of discontinuity. For, according to his view, fluctuations, that is to say the smaller variations which are constantly going on within a species, the minute differences between parents and children, the characters which distinguish one child from another—these things never do and never can produce a new species, even a new elementary species. "Fluctuations are linear, amplifying or lessening the existing qualities, but not really changing their nature. They are not observed to produce anything quite

new, and evolution, of course, is not restricted to the increase of the already existing peculiarities, but depends chiefly on the continuous addition of new characters to the stock. Fluctuations always oscillate around an average, and, if removed from this for some time, they show a tendency to return to it. This tendency, called retrogression, has never been observed to fail, as it should in order to free the new strain from the links with the average, while new species and new varieties are seen to be quite free from their ancestors, and are not linked to them by intermediates." Again, it must be insisted how important this question of fluctuations is to present-day ideas of evolution, since the theory of the accumulation of small changes underlies the ideas of both the two chief opposing camps of biologists, the Neo-Lamarckians, who assume a modifying agency on the part of the environment, and the Neo-Darwinians who refuse to accept this view. De Vries, Bateson and those who agree with them are consequently in opposition to both these groups, and deny a doctrine fundamental to both their creeds. De Vries argues that the variations on which both these schools of thought have relied are wholly different from what he calls mutations, and believes to be alone capable of producing new elementary species. Mutations are characterized, according to his view, by the "production of something new, by the acquirement of a character hitherto unnoticed in the line of their ancestors. On the contrary, varieties, in most cases, evidently owe their origin to the loss of an already existing

character, or, in other less frequent cases, to the reassumption of a quality formerly lost. Some may originate in a negative way, others in a positive manner, but in both cases nothing really new is acquired." (p. 247).

We have now to examine the evidence on which this far-reaching theory is built up, for far-reaching it certainly is. If it be established that by mutations alone is the development of a new species possible, then Darwin's doctrine, or a large part of it, absolutely falls to the ground. Darwin, as we have seen, insisted that natural selection worked only through the preservation and accumulation of small inherited modifications. Some of his followers have even contended that these modifications were so small that they could not be appreciated until natural selection had taken hold of them and made them obvious by a process of adding change to change. "It is only natural selection which accumulates those alterations, so that they become appreciable to us and constitute a variation which is evident to our senses."* But beyond this Darwin also asserted that his theory would "banish the belief of the continued creation of new organic beings or of any great and sudden modifications of their structure." But apart from this purely negative effect, certain positive results follow. Species regain something of the dignity which they formerly possessed, and the fact that mutations produce new species is a demonstration that the cause of the variation lies deep in the nature of life; that it is not fortuitous,

* Claus, *Textbook of Zoology.*

as it would be on the Darwinian theory, but that, as Dwight points out,* "it implies the existence of a type and of a law which under certain conditions becomes operative."

I shall have to refer further to this matter at the end of this article, and will therefore pass to two other points of great importance which come into direct bearing with the question of mutations.

In the first place one of the great difficulties which has always stood in the way of the acceptance of the transformist doctrine has been the inadequacy of the time which seems to have been at the disposal of the world for the process. On this point physicists and evolutionists have always been at issue since Lord Kelvin made public his views as to the age of the earth, the biologists demanding, for the operations which they supposed to have taken place, a length of time which the physicists were quite unwilling to concede. Now it is obvious that if the process of transformation can be shown to have taken place by great and sudden changes, the difficulty here alluded to very largely disappears. A comparatively short time might suffice for the accomplishment of changes through mutations, which would require untold æons if they were to take place through the slow accumulation of small variations.

In the next place the question of the geological record is very much simplified. When the Darwinian hypothesis was first made public, the common argument urged against it was the fact that as a matter of experience, the links which were

† *Science*, N.S. xxi, 529.

postulated had not been found to exist. And the equally common rejoinder was that our imperfection of knowledge as to the geological record was the complete and sufficing explanation of this apparent discrepancy between theory and fact. A number of years have now passed by, years during which unremitting labour has been carried on in the geological field, with, one must admit, surprisingly little effect from the point of view now under consideration. The pedigree of the horse, which, as has been already pointed out in a previous article, is far less clearly demonstrative of the facts which it has been brought forward to prove than it was originally supposed to be, still remains the commonplace of popular books and essays on evolution. It would certainly not occupy its proud position if any more striking examples of the same kind had come to light during the time that has elapsed since this history was first brought forward as proof positive of the truth of evolution. It still remains true—indeed, the truth becomes more obvious the more the crust of the earth is examined—that the geological record of life is as discontinuous as the discontinuous picture of living nature presented to our eyes to-day. Now this is just what one would not have expected under the Darwinian hypothesis, whilst, on the other hand, it is just what one would have expected had the course of development proceeded by sudden leaps and not by minute accretions of changes.

Such then are some of the important bearings which de Vries' views have upon the Darwinian

controversy. I must now devote a short space to explaining, in as simple language as possible, the facts upon which his theory is based.

In the first place it seems to be quite clear that, amongst plants at least, new elementary species are produced, and that the history of some of these is quite well known and quite unmistakeable. De Vries gives several instances, one of which will suffice. It is the case of the cut-leaved variety of the greater celandine, a plant which will be familiar to everybody with any knowledge of wild flowers. This species, *Chelidonium laciniatum*, was first seen in 1590 in the garden of one Sprenger, an apothecary of Heidelberg, who had for years cultivated the ordinary *Chelidonium majus*, or greater celandine. Sprenger recognized that something new to him had appeared in his garden, and sent specimens of it to the leading botanists of the day, including the celebrated Caspar Bauhin. All agreed that it was a new plant. Much interest was felt in it, and it was introduced into most of the botanical gardens of Europe. At the same time botanists—and whatever may have been the state of other sciences, field-botany then was in a more flourishing state than it now is—made search for this new plant in a wild state, but without any success. It seems to be quite clear that it actually did arise for the first time in the year and at the place mentioned. From the botanical gardens it spread to ordinary gardens, and from them it escaped and became a wild plant, and is now almost as common in many places as the older great celandine from which it originated.

De Vries has, moreover, made his own observations on a plant in the neighbourhood of Amsterdam which is, or was recently, in the condition of sending off new lateral species. This plant is the well-known large-flowered evening primrose *Œnothera Lamarckiana*, which is supposed to have come from America, though this point is doubtful, and to have been introduced into the gardens of the Musée d'Histoire Naturelle, at Paris, by no less a person than Lamarck himself, after whom it is named. It is one of those plants which freely "escapes," to use the botanical phrase, and is often to be found, therefore, in a wild condition. It was in such a state, at Hilversum, near Amsterdam, that de Vries came across a number of these plants. But the remarkable thing was that in addition to the well-known and unmistakable *Œ. Lamarckiana* there were a number of other varieties or species growing with it. These species are not only new but appear to be perfectly constant, so that here again we seem to be able to record the birth into the world for the first time of a new species. For the exact description of these new species the botanical inquirer must be referred to the pages of de Vries' work. There too he will find an account of the experiments of that botanical observer on a peculiar variety of species of toadflax, the peloric variety.

From these and other observations described at great length in his book he deduces the conclusions that new elementary species appear suddenly and without any intermediate steps leading up to them; that the new forms spring laterally,

so to speak, from the main stem, and are not necessarily advances or regressions; that they attain their full constancy at once, and that they exhibit just the same fluctuations and variations as the older species have. "But," he says (p. 569), "such oscillating changes have nothing in common with the mutations. Their essential character is the heaping up of slight deviations round a mean, and the concurrence of continuous lines of increasing deviations, linking the extremes with this group. Nothing of this kind is observed in the case of mutations. There is no mean for them to be grouped around, and the extreme only is to be seen, and it is wholly unconnected with the original type. It might be supposed that on closer inspection each mutation might be brought into some connexion with some feature of the fluctuating variability. But this is not the case."

Take, for example, the dwarf form of *Œnothera*. This is a distinct species. But there are larger and smaller forms of the ordinary *Œnothera*. No doubt, but the small forms never approach the size of the dwarf species. There is always a gap; and, what is more, the small examples of the ordinary species are weaklings whilst the dwarf species itself is remarkably robust.

It will be remarked that the observations on which de Vries' theory is founded are entirely drawn from the plant world; and so far no one, we believe, has pointed out any similar occurrences in the animal kingdom. On the transformist hypothesis, however, the two are continuous, and what one finds in the one should be discoverable in the

other. If it turns out that this is not the case, then another great difficulty will have been raised. But so far as botanical examples are concerned there seems to be good evidence that mutations attended by the production of new species are not of very uncommon occurrence. De Vries thinks that a species has periods at which it exhibits a remarkable tendency towards the production of new species. Such must have been the condition of the Hilversum *Œnothera*. At other times he thinks that long periods may elapse during which no mutations occur and no species are, therefore, given to the world. If this be true, and there seems to be a good deal of evidence for it, it points with the utmost clearness to one conclusion, which, indeed, is indicated by this whole matter of mutations, as by many other arguments which need not here be particularized. It is quite clear that the plant must have within itself a tendency to vary and to vary in certain directions, a force which enables it to make those sudden and complete mutations which have been described in this paper. It is not the environment which provides them or even calls them forth, so far as we can at present see. It is an inherent function of living matter, a function which we can appreciate without in any way understanding it, a function whose laws we can only guess at. That natural selection may come into operation after this function has been exercised is possible; that it has anything in the world to say to the causation of the function, as some have seemed to imagine, is obviously and entirely absurd.

V: MENDEL AND HIS THEORY OF HEREDITY *

IN the case of the former writers whose work has been dealt with in previous articles the life of the individual in question has not seemed of sufficient importance or significance to make it worth while to devote any space to biographical details. But the life of Abbot Mendel is of such peculiar interest, and the story of his achievements is of so unusual a character, that it may not be out of place to deal briefly with it before proceeding to consider the theory with which the name of its author is associated.

Gregor Johann Mendel, then, was born, the son of a farmer, in Silesia, in the year 1822. He was educated at Olmutz, and entered, at the age of twenty-one, as a novice in the Augustinian monastery of Königenkloster, in Altbrünn. He was ordained priest in 1846, became a teacher in the Realschule in Brünn, and attracted so much attention in that capacity that he was sent by his superiors to Vienna in 1851 to pursue a post-graduate course. After two years' study there he

* *Mendel's Principles of Heredity.* By W. Bateson. Cambridge University Press. 1902.
Mendelism. By R. Punnett. Cambridge: Macmillan & Bowes. 1905.
Recent Progress in the Study of Variation, Heredity and Evolution. By R. H. Lock. John Murray. 1906.
Catholic Churchmen in Science. By J. J. Walsh. The Dolphin Press. 1906.

returned to his Abbey, where he passed the remainder of his life, during the last sixteen years of which he held the position of abbot. No doubt he made an excellent head of the Abbey, but the labours thus cast upon him must have seriously interfered with his scientific studies, most, if not all, of which date from an earlier period. He died in 1884, at the age of sixty-two, and left behind him the still remembered recollection of one who, in addition to his great scientific merits, was possessed of a truly lovable personal character. Darwin died in 1882, so that the two workers were contemporaries; but it does not appear that the great Englishman ever heard of his Continental colleague, whose work, according to no less an authority than Professor T. H. Morgan, was destined to give the final *coup de grâce* to the doctrine of natural selection. Mendel's discovery was published in 1866 in the Transactions of the Brünn Natural History Society, and for thirty-three years it lay apparently still-born. But in 1899 three workers, de Vries in Holland, Correns in Germany and Tschermak in Austria, re-discovered the facts which Mendel had first brought to light; his paper was resuscitated, and the discoverer—a circumstance not of invariable occurrence—came into the reward of his labours.

In this country Bateson of Cambridge has been most active in sustaining Mendel's hypothesis; and he, Miss Saunders and Messrs. Lock, Punnett and Hurst have published numerous papers bearing on it under the ægis of the Royal Society and elsewhere.

That the theory in question is one of far-reaching importance may be gathered from the fact that Bateson has declared that "his [Mendel's] experiments are worthy to rank with those which laid the foundation of the atomic laws of chemistry," and that Lock claims that his discovery was "of an importance little inferior to those of a Newton or a Dalton."

Though later experiments in connexion with this matter of hybridity have been conducted in connexion with many kinds of animals as well as plants, Mendel himself was occupied with plants alone, and his most important observations were made on the garden pea, *Pisum sativum*. What these observations were, and what conclusions they have led to, it will now be the business of this article to detail. The main theory is not difficult of comprehension, but the intense research which has lately been directed to this subject has led to many complicated side-discoveries and has necessitated a nomenclature—here purposely omitted—which is a little difficult of comprehension to those who have not been brought up to biological studies. To begin with, then, Mendel's first innovation was the examination, not of plants as a whole, but of isolated pairs of characters belonging to certain plants. For example, in his study of the pea he found that there were some which had a greenish colour of the endosperm, or substance of the part of the pea which one eats, whilst others were yellow. Some were tall, others were dwarfs; some had round, ripe seeds, others angular and wrinkled. For the purposes of his experiments he

selected seven such pairs of characteristics and observed the way in which they were distributed in cases of hybridization. A concrete example will make the matter clearer than abstract description, and we will select one which has been brought forward by Bateson,* and which is of easy comprehension by all. There are both tall and dwarf, or "Cupid," sweet peas, these two kinds thus affording, in this particular, a very striking and easily-disinguished difference. Suppose now that we cross the tall with the dwarf variety, secure the resulting seeds and sow them, what results? All the plants which grow up belong to the tall variety. It might be supposed that the tall variety had simply wiped out the dwarf strain, but a further experiment shows that this is not the case. Let the tall children of the mixed tall and dwarf parents be self-fertilized; let the seeds thus obtained be sown, and it will be found that the resulting plants are mixed in character, and mixed, too, in definite proportions. For it will be found that on the average there are three tall specimens for every one of a dwarf nature. It would appear then that the dwarfishness was only hidden in the children; that its absence was apparent and not real, and that the potentiality was there in the germ and made itself evident in the grand-children. To the character which alone appears in the first cross is given the name "dominant"; to that which, existent in one of the original parents, hidden in the children, becomes again obvious in some of the grand-children, is given the name

* "Mendelian Heredity," *British Medical Journal*, July 14, 1906.

" recessive." Let us again allow the talls and dwarfs thus obtained to be self-fertilized, and a remarkable result follows. All the recessives (or dwarfs) breed true, and, to make a long story short, it may be added, will continue to breed true, that is, to produce dwarf forms without any admixture of the larger variety for any number of generations. Such is not the case with the dominants. They, when self-fertilized and sown, produce both talls and dwarfs. Some of the talls will be pure, others will not, for their offspring will give both varieties, and the pure are to the impure on an average as one to two. Hence, out of the first hundred plants, seventy-five will be dominants, or, in this case, talls, and twenty-five recessives or dwarfs. But the latter will be pure and will go on, so long as they are not crossed, producing dwarf specimens. Of the seventy-five dominants twenty-five will be pure dominants and will go on producing talls, but fifty will be mixed, and their progeny will consist again of pure dominants, mixed dominants and recessives as above stated.

The laws which underlie these observations have been formulated by Professor Davenport,* whose statement is here reproduced.

" The two great laws enunciated by Mendel were these : Of the two antagonistic peculiarities possessed by two races that are crossed, the hybrid, or mongrel, exhibits only one ; and it exhibits it completely, so that the mongrel is not distinguishable as regards this character from one of the

* *Science*, N.S. vol. xix, no. 472, pp. 110-114.

parents. Intermediate conditions do not occur. That one of the two parental qualities that alone appears in the mongrels is called dominant; the other recessive. Second, in the formation of the pollen or egg-cell the two antagonistic peculiarities are segregated; so that each ripe germ-cell carries either one or the other of these peculiarities, but not both. It is a result of the second law that in the second generation of mongrels each of the two qualities of their grandparents shall crop out on distinct individuals, and that the recessive quality shall appear in twenty-five per cent. of the individuals, the remaining seventy-five per cent. having the dominant quality. Such recessive individuals, crossed *inter se*, should never produce anything but recessive offspring."

We have already seen that it is one of Mendel's claims to distinction that he introduced to biologists the idea of unit characters, which can be inherited independently of one another, but his work leads to further reaching considerations than this. In a previous article it was shown that de Vries and others claim that by transmutation alone can different species have arisen. In other words they claim that development is not Continuous, as Darwin believed, but Discontinuous, just as the face of nature presents us with discontinuous species. Now Mendel's observations show us that there is Discontinuity in Inheritance as well as in Variation. It was once asserted that a mutation was in danger of being swamped by inbreeding with the normal form, but Mendel has shown that this

was not the case. And so, says Mr. Punnett (p. 55):

"The position of the biologist of to-day is much the same as that of the chemist a century ago, when Dalton enunciated the law of constant proportions. In either case the keynote has been Discontinuity—the discontinuity of atom, and the discontinuity of the variations in living forms. With a clear perception of this principle, and after a long and laborious period of analysis, the imposing superstructure of modern chemistry has been raised on the foundation of the atom. Not otherwise may it be with biology, though here perforce the analytical process must be lengthier, both from the more complex nature of the material, and from the greater time involved in experiments on living forms."

Characters of colour are not difficult to study, and a large number of the recent researches into Mendelian principles have dealt with that class of observation. Thus, according to Lock (p. 200) "colour characters which follow Mendel's law have been observed in mice, rats, rabbits, guinea-pigs, pigeons, fowls, cats and so on. In butterflies and other insects, and even in snails, similar phenomena have been described." An interesting point is that in plants possessing both coloured and white varieties, so far as investigations have been pursued, the white forms are recessive to the coloured. Yellow-seeded maize is dominant over the white-seeded variety, and the so-called "sugar" is recessive to the "starch" variety.

Two remarkable instances, one from the animal, the other from the vegetable kingdom, may be cited before passing to some further considerations. There is a curious kind of mouse, known as the Japanese "waltzing mouse," which gets its name from the fact that it will spend hours in the day chasing round and round after its own tail. It is known that this curious habit is the result of a malformation of the semicircular canals, passages connected with the internal ear and having to do with the balancing power of the possessor. Experiments were made by breeding "waltzers" with normal mice, the result showing that the malformation in question functions as a unit in comparison with the normal condition and that it behaves as a recessive. Thus, to make things quite clear, the offspring of a "waltzer" and a normal mouse is always normal, but in the next generation "waltzers" reappear.

Readers of Darwin's books will be aware that he devoted much attention to the question of the two kinds of primroses which exist in nature in about equal numbers. In one of these varieties, which is called "thrum," the anthers are in the top of the tube and the style is short. In the other, or "pin-eyed," the anthers are at the point of contraction of the tube and the style is long. These two characters also behave as Mendelian units. The "thrum" is the dominant form, and long-styled plants, being recessive, never produce short-styled, unless they have been fertilized from a flower of the other variety. As Bateson points out, this fact goes a long way towards disposing of

one of the objections which was brought against the Mendelian theory when repropounded, namely, that wild animals and plants might not follow the rules applying to domesticated breeds. Also it clears up, so far as such a thing can ever be cleared up, a difficulty to which Darwin devoted a vast amount of time and space.

It is quite clear, from what has been said, that a knowledge of the Mendelian laws, assuming that they are proved to be laws, ought to be of the highest value to breeders and to agriculturists. To the former the need of securing a pure strain has always been of paramount importance. If the Mendelian laws be true, then the problem will be to find out the unit characters, establish which is the recessive variety, and in that will be found a pure race. A pure dominant race can also be obtained, as has been shown. A few instances of the application of these laws to purposes of the kind indicated above will be of interest.

The variety of fowl known as the Blue Andalusian has long been the despair of breeders. Do what they might, select as carefully as they could, it seemed to be impossible to secure a pure strain of this form. No matter how perfect the parents, the results have always been the production of "wasters" of two kinds, some of them being pure black, and others of a peculiar white, with black splashes. On the average the results of the breeding of a pen of Blue Andalusians is the production of twenty-five per cent. each of black and of splashed, and fifty per cent. of blue. This fact receives a perfect explanation on Mendelian lines.

The black and the splashed are really pure races, and behave as such when bred. The so-called "pure" Blue Andalusian is a mongrel and must always remain such. In fact, experiment has now shown that more Blue Andalusians can be got by mating black with splashed "wasters" than by mating birds of the blue colour. There is a remarkable point about this case, and that is that the mongrel does not resemble either of its parents. There is, therefore, neither dominant nor recessive in this case, and the Mendelian law is to some extent departed from. This is a point which will be dealt with later on; it is, no doubt, one which will be cleared up on further investigation, and this line of inquiry may lead to a wider extension of the law or laws which govern hybridity.

Even more promising experiments have been made in connexion with wheat, and two of these may be described, as they show the vast importance which the discovery of the Augustinian abbot may have upon agriculture. Our first instance relates to a certain quality known as "strength," a quality which, it appears, is absolutely essential if the flour produced is to be finally made into the only kind of loaf which is saleable at the present time in England. So far this quality has been absent in those kinds of wheat which it has been found possible to grow at a profit in this country. It is present in American and Canadian "hard" wheats, and hence these are worth some shillings a quarter more than the English corn, another difficulty in the path of the already sufficiently depressed corn-growing industry of England. It

may be asked, Why not grow these "strong" American varieties in this country? That, of course, has been tried, but the result shows that nearly all lose their quality and become as "weak" as the English wheat. Some do not, and of these the Red Fife is an example. But then these "strong" wheats yield in England a smaller crop per acre, and as a result the increased price which is obtained for them does not make up for the diminished amount which is produced. Here was a case in which experiments on Mendelian lines might well be made, and they were conducted by Mr. Biffen at the Experimental Farm of the Cambridge University Agricultural Department. For this purpose Manitoba Hard, a "strong" wheat, was crossed with a typical English wheat—Rough Chaff. The result was the production of plants all possessed of hard grains. The second generation produced "strong" and "weak" grains in the proportion of three to one. This, as we have seen, is the classical Mendelian proportion, and it became obvious that the weak was the recessive, the strong the dominant character. But, as we have already seen, in later generations certain of the dominants will be pure forms, and when these have been established, as can be done by several series of growings, individuals will have been produced which will combine strength of grain with the other desirable qualities of the second parent. "The problem has, therefore, been completely solved, and there can be little doubt that when these new types are brought into general cultivation the profit obtainable from the growing of

wheat in this country will be increased by several shillings to the acre of crop grown." *

A further, perhaps even more important, experiment has been made in connexion with the power to resist the attacks of "rust," a fungous disease due to the attacks of *Puccinia graminis*, the loss caused by which is said to amount to a considerable number of millions of pounds sterling per annum throughout the world.

There are some strains of wheat which resist the attacks of the enemy, but unfortunately they are strains which, for other reasons, are not very suitable for cultivation. Mr. Biffen again attacked this problem and with success. He took a form which was always immune, even when grown in the midst of a field of rusted plants. He took another form, known as Michigan Bronze, which seems never to escape the plague, and he crossed the two together. Mr. Lock gives the result (p. 220):

"In the first generation every plant without exception was badly rusted, but fortunately a considerable number of ripe grains was obtained, and these were sown to produce the second generation. When the plants of this generation had grown up, it was observed that among a majority of badly-rusted plants certain individuals stood our fresh and green, being entirely free from infection. On examination it was found that every plant could be placed in one or other of two categories—either it was badly rusted or it was

* Lock, p. 219.

entirely free from rust; and the numbers of the two kinds of plants were as follows: 1,609 infected, 523 immune. It is clear, then, that immunity and susceptibility to the attacks of yellow rust behave as a simple pair of Mendelian characters, immunity being recessive. And it is, therefore, possible to obtain by crossing, in three generations, a pure rust-free strain containing any other desired quality which is similarly capable of definite inheritance."

If these observations, as certainly seems likely, come to be ultimately established beyond cavil, the Augustinian abbot's observations in his monastery garden will have had for their result the saving of vast sums annually for the agriculturists of all countries.

In speaking of the Blue Andalusians mention was made of the fact that to some extent they departed from the original Mendelian laws, in that a new form not represented by either of the parents was produced, and it was suggested that some explanation of this point was probably attainable. Some indication of how this will be arrived at may be gleaned from experiments which have been made with red and cream-coloured stocks and other flowers. Bateson describes these experiments and says:

"The red variety is characterized by red sap, the cream variety is characterized by yellow corpuscles, surrounded by colourless sap. In the red those corpuscles are represented by colourless corpuscles floating in the red sap. What will

happen when these two are crossed together? As a matter of fact we find that the red is dominant. But in the second generation we have nine red, three red with cream, three *white* and one cream as before. What is the white? White was not put in. Apparently we have produced it *de novo*—an albino by crossing two coloured forms: cream was a corpuscle colour, red was a sap colour, but the white have *colourless* corpuscles floating in *colourless* sap. It is evident what has happened. Which are the factors which segregate in the formation of the germ cells? They are (*a*) red sap from colourless sap, and (*b*) white corpuscles from yellow corpuscles; so that when the possible combinations of those two pairs of characters are made, colourless corpuscles may coincide with colourless sap, and a white flower is the result. I think the answer is quite clear." *

Sufficient has now been said to show what Mendel's law is and the important results which may flow from it; but it would not be fair to conclude without stating that there is still an important body of biologists who refuse to believe in the truth of the theory, and severe, even acrimonious, contests have been waged between the upholders and the deniers of Mendelian views. The chief opponent was the late Professor Weldon of Oxford. To discuss the exact points at issue would occupy more space than can be afforded in an article of this kind, of which the main object is to describe Mendel's theories and some recent

* *British Medical Journal*, ut supra.

work following on them. Suffice it to say that Professor Weldon was of opinion that sufficient attention had not been paid to the ancestry of the forms experimented with; and here it may be noted that de Vries—one of the re-discoverers of the theory—is of opinion that Mendel's law of dichotomy only holds in general for phylogenetically recent characters. Those who desire to pursue their investigations further must consult the works placed at the head of this article and others to which they will find themselves referred in the books and papers cited.

It may, however, be stated that some workers have, in their experiments, been led to the conclusion that the Mendelian laws are not everywhere and in all cases applicable. Thus, to give but two examples, Davenport concludes that "while Mendelian principles seem applicable to some cases of crosses between sports and the normal species, there seem to be others where neither Mendel's nor Galton's Law of Inheritance holds." *

This conclusion he arrives at from his study of the so-called "wonder-horses." Moreover, his study of colour in mice leads him to think that "Mendel did not discover all the important laws of inheritance"—which is more than likely—"and that further investigation will unquestionably reveal other and still broader principles of heredity." Again Toyama,† as the result of a prolonged examination of silk-worm crosses,

* *Science*, N.S. vol. xix, no. 473, pp. 151-153.
† *Biologisches Centralblatt*, Juni 1, 1906.

concludes that whilst some strictly follow Mendel's laws, others obey certain other laws which are not to be exactly formulated as those of Mendel.

One last instance may be given to show the remarkable differences of opinion which may be held by men of science as to the interpretation of the same set of facts. Mr. Hurst, a well-known worker and an adherent to Mendelian principles, and the late Professor Weldon, an equally stout opponent of the same, both conceived the idea of studying the inheritance of coat colour in horses. Obviously to work this out by the ordinary method of experiment would take at least one long lifetime, not to speak of great expenditure of money, so both of them had recourse to the pages of a work known as Weatherby's *General Studbook of Horses*. It appears that this *magnum opus* runs to twenty volumes, and gives "fairly complete records of the age, colour, sex and parentage of British thoroughbreds from the earliest accounts down to the end of 1904." Their conclusions will be found in the Proceedings of the Royal Society. * Both, as has been stated, dealt with the same series of facts, and both arrived at opposite conclusions. Mr. Hurst thought that the Mendelian laws had been justified; Professor Weldon was of opinion that they could be shown not to apply. No doubt in this case the evidence is second-hand. The colours of the race-horses dealt with were given for other than scientific reasons, and probably no one would have been more surprised than the estimable Mr. Weatherby, or whoever may be

* Series B, vol. LXXVII.

responsible for the production of the vast work which bears his name, to find its pages the battle-ground of two men of science. Such facts are, of course, of much less value than those obtained in the experimental garden or breeding-ground, but that such divergence of opinion is possible, even on such a series of observations, shows the difficulty of coming to a final judgment on matters of this kind.

At least this must be said for Mendel's law or theories. They have opened up new lines of investigation, and have—so far as one can see—established new laws of relation between parents and offspring. And, not least, they have given to breeders and agriculturists a hint as to the direction in which their pursuits may be most profitably and successfully pursued.

What is really wanting in this matter is an abundance of carefully conducted observations. When one considers the interest, not to say the fascination, of the subject, and when one remembers that it is in the power of anyone who has a modest greenhouse, or even an ordinary garden, to carry out such observations for him or herself, it is somewhat wonderful that more has not been done towards the elucidation of the remarkable problems which, from the obscurity of his monastery, the Augustinian abbot set to the world.

VI: THE FORM OF THE HUMAN SKULL

THE form of the human skull is a matter which has engaged the attention of anatomists for many years: with increasing insistence since Blumenbach, in the latter part of the eighteenth century, published his *Decas Collectionis suae Craniorum Diversarum Gentium;* and is now, in these our own days, exciting a new and ever-enlarging interest. There is little cause for wonder in this when one reflects upon the fact that the shape of the human skull has, or is supposed to have, a very marked bearing upon that branch of the evolutionary question which deals with the origin of man's body The skull, that is the cranial portion as distinct from the facial, is the enveloping capsule which protects the brain—the central organ of the nervous system—and the size, at least of the latter, can with some certainty be calculated from the capacity of the shell in which it lay. Hence a vast amount of attention has been paid to the examination of the skulls of the lower races, to the skulls of pre-historic men and women, and particularly to those few battered fragments which at present we regard as having belonged to some of the earliest denizens of this planet. It is no part of the intention of this article to deal with the question of evolution in its larger bearings, nor even of the evolution of man's body, the corporeal prison which encages his soul, and through

the imperfect medium of which that soul strives to make itself known. It is no part of my intention to discuss this matter from its biological, its philosophical, still less from its theological aspect, but to occupy myself with a much less extensive tract of country. In a word, I propose to examine the craniological question and the views of experts upon it; and having brought forward these statements which bear most weightily upon it, to consider what light they shed on this particular question. I do not intend to discuss the larger question. I propose to ask myself simply, "How far do the facts which we have before us—here and now—bear upon the question? Are they for or against the probability of the evolution of man's body? Or do they, as far as they go, leave the matter in a wholly undecided condition?" Father Gerard, S.J., in an article in *The Month* on the subject of "Free-Thought," says that his opposition to the Darwinian theory, as commonly accepted, is on scientific grounds, because, as it seems to him, the evidence is all against it. And he goes on to add: "I have no antecedent quarrel with it, and am ready to accept it at once if it can only be shown to explain the facts. There can be no philosophical or theological objection to it which I can imagine." What we want, as Fr. Gerard says, is a sifting of the facts. We have had a terrible lot of theory-spinning of late, justifiable much of it, at least to a large extent, but I think it is time to cry out for a halt for the examination of spoils and the estimation of their real value. We are accustomed to have it dinned into our ears, often by

persons who are perfectly incompetent to pronounce any opinion upon the subject, that such and such things are proved facts of science. Well, are they? Or are they merely more or less plausible theories? These are the questions which I think we might well sit down and consider in connection with a good many things which some have been disposed to take for granted. With so much by way of introduction, let me now turn to the subject with which this article is mainly concerned—the form of the earliest known skulls. Before attacking this point, however, or rather in the course of our attack upon it, it will be necessary to clear the ground by considering briefly some of the methods which have been and are being adopted for the purpose of comparing different skulls and estimating and measuring the differences which exist between them. When we have studied these methods we can then consider certain points which arise in connection with the relations between the size of the skull and the size of the brain which it contains. We shall then be in a position to understand the bearing of the observations which have been made upon the group of skulls which we are to consider. In the first place, let me make it clear that I am dealing solely with the cranium proper, that is, the brain-box, and not with the accessory group of bones which forms the face, constitutes capsules for the organs of sight, smell and taste, and has to do with mastication. There are many interesting problems connected with that group of bones, but these must be left aside, for to treat of them in any way would unduly

and unnecessarily add to the length of this article.

The first point which an observer would wish to ascertain about any given cranium is the actual measure of its capacity, for this, when once obtained, affords us a means of estimating the size of the brain which it contained. I need not tediously describe the process by which this figure of capacity is arrived at. Suffice it to say that, after all the holes leading from the cranial cavity, with the exception of the foramen magnum (through which the brain and spinal cord communicate), have been stopped up, shot or millet seeds or the like is poured into the interior of the skull and well rammed down, until the cavity is quite full. It is then an easy matter to empty the shot or seeds into a large glass measure and ascertain the capacity of the skull in cubic centimetres.

What do we learn from the measurements thus obtained? First of all we learn that amongst perfectly normal individuals in the human race the capacity may vary to the extent of double the minimum figure, that is to say, from 1,100 to 2,200 cubic centimetres, so that there is a very great range of variation in this particular.

About 1,550 cc. may be taken as the average capacity of European, and indeed of Asiatic, races. The average for Negro and Oceanian races is about 100 cc. smaller. Still smaller are the skulls of Australians, Bushmen and Andamanese. Indeed, the average of a group of skulls of Andamanese women only amounted to 1,128 cc. But here one has to pause and consider two points. First, one

has to remember that the actual stature of the individual must be considered in its relation to the size—and, of course, therefore to the capacity—of the skull. A man of six feet four inches would look as absurd if he bore the head suitable for a woman of five feet as the latter would with the skull of her taller brother. Australians are persons of average stature, and their skulls are relatively somewhat small, but the inhabitants of the Andaman Islands are amongst the smallest races—not actually pigmy—now in existence. The average stature of the Andaman male is four feet six inches, *i.e.*, the height of a boy of ten or twelve years of age. Such a stature would not consort with a skull of average European size. What is remarkable, however, is that the Andaman skull is relatively larger in comparison with the stature than the European skull, though it is absolutely smaller. To be exact upon this point, it may be mentioned that the length of the head, from vertex to chin, is contained seven and a half times in the stature of an average European; whilst it is contained only seven times in the stature of an Andamanese. Hence his skull is relatively larger than that of a European. When we come to actual pigmies, such as those discovered and described by Stanley, where the stature varies from three feet to four feet six inches, the skull is still smaller; and though I know of no measurements of capacity which have been made on any skulls of this race, their cranial cavity must be considerably smaller even than that of the Andamanese. As regards the skulls of more ancient races, it is often difficult or even impossible

to ascertain the cubic capacity, because of the imperfection of the remains. But the capacity of a group of skulls belonging to the neolithic * period in France, from the cavern of l'Homme Mort, in the Lozère, gives us as the following measurements, which for the sake of contrasts may be compared with the measurements made of skulls of ninteenth century Parisians by the same observer Broca :

	No.	Men.	No.	Women.	Difference
l'Homme Mort	6	1606 cc.	6	1507 cc.	99 cc.
Parisians (xix c.)	77	1559 cc.	41	1337 cc.	222 cc.

From these figures we learn two things : first, that the modern Parisian skulls are actually smaller in size than those of the persons who occupied the same country at a very much earlier period, which is a striking enough fact. In the next place, they bring out the great difference in size which exists between the skulls of men and women, one of numerous secondary sexual differences, and one of the most striking of these differences. Of course to a great extent, almost entirely in fact, this difference in cubic capacity between the two sexes is accounted for by differences in stature ; but when we consider that, so far as these figures can

* Although these questions are more fully dealt with in a later article the following note may be advisable at this point. Since we know of the existence of early races of men almost exclusively through the implements which they have left behind them, the terminology is founded, in the first instance, on these implements, and so we have a Stone Age, followed by a Bronze Age, and that again succeeded by an Iron Age. The Stone Age is divided into two—or sometimes three—main sub-divisions ; a palæolithic or old stone age, during which implements were rough-hewn and never polished ; and a later, neolithic, or new stone age, during which implements were sometimes, though not always, polished.

teach us, the average difference between the skull of a Parisian man and a Parisian woman is twice as great as that between the skull of a European and a Negro, and as great as that between a European and an Australian ; and when we admit, as we must, that the Parisian woman of the nineteenth century was not by any means lacking in brains, we begin to see that actual cubic capacity, within very wide limits too, does not much help us in gauging the intellectual capacity of the former owner of the cranium. Speaking of the Engis skull, then thought to belong to the Mammoth period, but now believed to be of much later date in fact, Huxley wrote : " It is, in fact, a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." And Deniker states that " the cranial capacity of lunatics, of certain criminals, and especially of celebrated or distinguished men, scholars, artists, statesmen, etc., appears to be slightly superior to the average of their race." So that we have a wide range for conjecture as to the character of its owner when we are confronted with a skull of more than ordinary cubic capacity, but we are not at liberty from that character alone, at any rate, to conclude that he or she was superior either in intellect or in morals to the possessor of some much smaller brain-case.

From measure of capacity we pass to measure of length. These indices, so-called, or expressions of one measurement in terms of another, are legion, and I shall content myself with dealing with two of them which are, for our present purposes, the

most important. If we take a series of skulls and look down upon them from the top or convex surface, from the *norma verticalis*, as craniologists call it, the most careless observer will scarcely fail to note that whilst some of these skulls—if a sufficiently miscellaneous lot has been chosen—are nearly as wide as they are long, others are much longer than they are wide. In a word, some heads are—literally, not metaphorically—long, some broad. To express this relation of length and breadth numerically is the object of what is known as "the horizontal cephalic index," or sometimes, from its paramount importance, "the cephalic index." The length is measured from the *glabella*, or surface above the root of the nose and between the eyes, to the *inion*, or external occipital protuberance, a knob of bone at the back of one's head which every person can easily make out. Then the greatest breadth is taken—usually just above the ears. The last of these measurements is multiplied by 100 and then the result is divided by the length. The following formula indicates the process :

$$\frac{\text{Transverse Diameter} \times 100.}{\text{Antero-Posterior Diameter.}}$$

Those skulls which are much longer than they are broad are called dolichocephalic, the broad skulls are called brachycephalic, and the intermediate group mesocephalic. Different writers have assigned different limits for these three groups, but the following table may be taken as fairly representing the present state of opinion on this point :

Dolichocephalic	...	...	58.0	...	74.9
Mesocephalic	...	...	75.0	...	79.9
Brachycephalic	...	...	80.0	...	103.0

It will thus be seen that the extreme variations permit of the existence of a skull which is nearly twice as long as it is broad (Deniker), and of one which is actually broader than it is long (Ripley). Summing together a few points which have been gleaned from the consideration of facts learnt by means of this index we can, in the first place, state that all the most ancient skulls at present known to us are dolichocephalic. This is a somewhat striking fact, but it is not, as we shall see, correlated with any necessary intellectual inferiority in long-headed races of persons.

Then, in the next place, we can state that the different varieties of shape are not confined to special parts of the world, but are more or less scattered and intermixed. Huxley, in his picturesque and striking manner, stated that the subject of the distribution of head-forms might be summed up in very few words. "Draw," he said, "a line on a globe from the Gold Coast in Western Africa to the steppes of Tartary. At the southern and western end of that line there live the most dolichocephalic, prognathous, curly-haired, dark-skinned of men—the true Negroes. At the northern and eastern end of the same line there live the most brachycephalic, orthognathous, straight-haired, yellow-skinned of men—the Tartars and Calmucks. The two ends of this imaginary line are indeed, so to speak, ethnological antipodes. A line

drawn at right angles, or nearly so, to this polar line through Europe and Southern Asia to Hindustan would give us a sort of equator, around which round-headed, oval-headed, and oblong-headed, prognathous and orthognathous, fair and dark races—but none possessing the excessively marked characters of Calmuck or Negro—group themselves." This striking generalisation must nowadays be corrected by rather more accurate figures, based on more extended observations. The general position may be summed up in two quotations from Deniker. "There is," he writes, "a certain regularity in the distribution of the different cranial forms on the surface of the earth. Dolichocephaly is almost exclusively located in Melanesia, in Australia, in India, and in Africa. Sub-dolichocephaly, diffused in the two extreme regions north and south of Europe, forms in Asia a zone round India (Indo-China, Anterior Asia, China, Japan, etc.), but is met with only sporadically in other parts of the world, especially in America. Mesocephaly is frequent in Europe in the regions bordering on the sub-dolichocephalic countries, as well as in different parts of Asia and America. Sub-brachycephaly, much diffused among the Mongolians of Asia and the populations of Eastern Europe, is very rare elsewhere. Lastly, brachycephalic and hyper-brachycephalic heads are almost exclusively limited to Western and Central Europe, to some populations of Asia, Turko-Mongols, Irano-Semites, and Thaï-Malays. And again, excluding the sub-classes, the nature of which will be readily understood without any

detailed explanation, and dealing only with the three main categories, he writes :

" We see that generally the crania of Negroes, Melanesians, Eskimo, Ainus, Berbers, the races of Northern Europe, etc., are dolichocephalic, while those of the Turkish peoples, the Malays, certain Slavs, Tyrolese, etc , are brachycephalic ; that the dolichocephalic predominate in Great Britain, while the brachycephalic are in a majority in France." Finally, it may be stated that, as these facts alone would be sufficient to show, the horizontal cephalic index tells us nothing about intellectual characters. The statement will be strengthened when we consider that the Chinese, a conspicuously long-headed race, by no means intellectually deficient, are surrounded by barbarian brachycephalic and very brachycephalic hordes, and that in many respects the very dolichocephalic Eskimo surpass the more brachycephalic Indians in culture. Nor is there any class difference, for in Italy Dr. Livi found that whilst in the northern part of the country the professional classes were longer headed than the peasants, a diametrically opposite condition prevailed in the south. Comparing brain-weight with skull-shape, Calori has shown that whilst among Italian men the brachycephalic have on an average twenty-seven grammes of brain more than the dolichocephalic, amongst Italian women the dolicocephalic have the better of the brachycephalic by twenty-one grammes. A curious example of the fact that the cephalic index tells us nothing about

intellectual capacity is to be found in the measurements of the heads of a group of thirty-six anatomical teachers in universities and medical schools which were taken at a meeting held in Dublin in 1897. Amongst these gentlemen were a few Germans and Belgians, as well as Irish, English and Scotch. Well, the result, so far as the horizontal index is concerned, is very curious. Amongst so few persons there is a range from 86.5, the most brachycephalic, to 72.2, the most dolichocephalic, and between those figures every gap is filled up, that is to say, there is no complete numeral unrepresented, some measuring 85 odd, some 84 odd, and so on to the lowest. As a matter of fact, fifteen in the list were brachycephalic, fourteen mesocephalic and seven dolichocephalic. The late Canon Isaac Taylor, in his book on *The Origin of the Aryans*, tried to show that the shape of the head had something to do with intellectual faculties, and even with the form of religion professed by its owner. He tells his readers that the dolichocephalic Teutons are Protestant, the brachycephalic French Catholic, and the mesocephalic Englishman neither one nor the other, but something between the two, which, as he is writing a scientific book, he does not try to define. Now the Canon was not right in his fundamental facts, for England is predominantly dolichocephalic, as Ripley has shown, and not mesocephalic; but if he had been right in this statement, the facts which I have brought forward will show how absurd and useless it is to attempt to gauge the intellectual characters of its owner by the length and breadth

of his skull. Even our popular phraseology is here incorrect, for if the balance inclines either way at all it is against the "long-headed" man and in favour of the "broad-headed."

A further index, which, being less important, will not be dealt with at such length, is the altitudinal, that of height to breadth or length. Without going into any minutiæ on this subject, it must be quite clear to the most casual observer that some persons have high, some low heads. In the language of craniologists, we have platycephalic or low-headed, hypsicephalic or high-headed, and orthocephalic or medium persons. Here again, within limits, the height of the head has nothing to do with intellectual characters. Within limits, I repeat, because there is a group of skulls called microcephalic, and housing brains of deficient character, in fact the brains of idiots. With this group I am not concerned. Microcephaly is a very interesting subject, and presents us with the important problem—still unsolved—as to whether the brain stops growing and therefore the skull does not expand but remains small, or whether the prime offender is the skull, which closes its sutures too soon and so prevents the brain from proceeding to its normal development. I must not allow myself to be drawn aside into the interesting points suggested by a consideration of the question of microcephaly, and I will, therefore, guard myself from misconception by stating that in all the remarks which I make with regard to the size and shape of skulls I am dealing only with those which may be regarded as normal for their sex

and race, and not with pathological skulls, to which class the microcephalic crania belong.

As I have previously said, the indices which have been invented for measuring and estimating different points in connection with the skull are legion, and when one reads papers like those of Schwalbe's on the Neanderthal or Trinil skulls one finds it at first very difficult to see the wood for the trees. Then, when one has got the focus and sees what is the outcome of this mass of measurements and figures, and how small, how highly disputable, and, therefore, how comparatively valueless it is, one is tempted to exclaim of indicial craniology: "All that I know is nothing can be known."

That the present state of craniology is largely one of pure chaos is my firm belief, and lest this remark be taken as a mere *ipse dixit* I will quote a very recent instance in proof of my statement. Professor Thompson, of Oxford, has just* published with a colleague a most elaborate work, containing measurements of 1,561 Egyptian skulls from the Thebaid and extending in period from the Pre-Dynastic races, through many of the Dynasties down to the Ptolemaic and Roman periods. It seemed desirable to separate off those skulls having a negroid character from those unaffected in this way, and the Professor lays down certain indices by which this may be done, and proceeds to separate his crania into two groups—negroid and non-negroid—by the help of these indices. Before his book had been given to the public a month a

* It will be remembered that this article appeared in 1905.

letter appeared—surely an unnecessarily unpleasant letter—from Professor Karl Pearson, another great pundit of the weight and measurement school of biologists, in the columns of *Nature*, in which that Professor writes: "Two distinguished Oxford men of science have given a very simple series of conditions by which crania can be classed into skulls of negroid, non-negroid and intermediate types. These conditions depend entirely on a classification of nasal and facial indices, and by their processes our authors are able to distinguish between the negroid, non-negroid and intermediate types among pre-historic Egyptian crania." Then he goes on to state that he has applied the rules to two groups of skulls: "First, to a fairly long series of admittedly negro crania, all males. I find that 7.3 per cent, are non-negroid, 39.0 per cent. are truely negroid, and 53.7 per cent. are intermediate. It is clear that we only need to let the negroes change their skins and that a sensible percentage will be non-negroid. Secondly to a fairly long series of English skulls, male and female, I find of Englishmen 20 per cent. are negroid, 46 per cent. non-negroid, and 34 per cent. are intermediate in type. Among Englishwomen 11 per cent. are negroid, 48 per cent. are non-negroid, and 41 per cent. are of intermediate type. Thus of the whole English population slightly more than 50 per cent. are either pure negroid or partially negroid; while in an outwardly pure negroid group, upwards of 60 per cent. are non-negroid or mixed with non-negroid elements." And he concludes with the ironical remark "that

the Englishman should have as large a negroid element in his constitution as the pre-historic Egyptian, and only half as little pure negroid element as admitted negroes," is to his mind " an epoch-making discovery." I do not think that one could desire a better example of the uncertainty and, therefore, of the uselessness of many indices as racial tests than that which I have just quoted.

It was possibly, in part, a feeling of this kind which induced Sergi, a distinguished Italian craniologist, to propose his natural system, which is based on the shapes of the skull, as viewed from the norma verticalis, which shapes he believes to be " persistent alike in geographical distribution and in the order of time, and therefore reliable elements for classification." Moreover, he claims that the " interior " or skeletal parts are " not affected by the external influences of habitat, climate, or nourishment." Hence it follows that his types are hereditary—which all would admit, within limits—and unalterable—which is a much larger postulate and one with which I cannot now deal. In any case this criticism may be made of Sergi's system. It is not hard for any trained observer to place a skull in its proper class according to this classification ; to indicate whether it is ellipsoid, pentagonoid, ovoid, beloid or sphenoid, if the skull is fairly well-marked in its shape. But to decide upon those on the borderland between these main shapes, or to go further and allot skulls to the numerous sub-classes laid down by Sergi, is a much more difficult process and one which must necessarily be tainted by that which we

should always strive to eliminate, namely, the personal factor. Professor Thompson admits this in the work from which I quoted a short time ago: "The refinements of some of his (Sergi's) classifications," he says, "are such that it would have been impossible for us to have grouped the [our] specimens without running the risk of, in some instances at least, falling into error." It will be admitted that a system which can only accurately be carried out by one man is of little use to the world of science, and in spite of the very ingenious method for geometrically getting over the difficulty which has recently been devised by Dr. Wright, Sergi's system is not at present one which affords much assistance to workers. But I am firmly of opinion that in this direction, and not in that of never-ending indices, lies the hope of craniology in the future.

The interest of the skull, as we have seen, is mainly due to the fact that it is the receptacle and fortress of the brain, and we may therefore turn for a few moments to a consideration of the relations between the two and to certain points which call for mention in connection with this matter. In the first place, it may be pointed out that whilst we have records of a large number of weights of European brains, we have very few, in comparison, of other races. There are records by Topinard of 11,000 brain-weighings amongst adult Europeans, and the next largest series, that of Negroes, only amounts to 190 examples. But we can in some measure get over this difficulty by obtaining the cranial capacity of the skull and

multiplying it by the co-efficient 0.87, first suggested by Manouvrier, by which process we obtain a reasonably accurate estimate of the weight of the brain.

Now from the various weighings and measurings we find that the average weight of the male European brain is 1,361 grammes and of the female 1,290. The weights of the Negro brains are not much lower. The difference ascertained by Manouvrier—from estimation of weight as deduced from cranial capacity—between modern Parisians and the lowest races examined was 196 grammes. But weighings even of individuals of the same race give very curious and conflicting results. For example, one observer from the weighing of twenty-eight brains obtained an average for the Englishman of 1,388 grammes. Another observer, from a series of 425 brains, obtained an average for the same race of 1,354 grammes. The difference between these two averages is greater than the differences between the average weight of the European brain and that of the brain of Annamites, as determined by a small series of actual weighings ; and it is not much less than the difference between the average European brain and the average Negro brain as determined by actual weighings. As a matter of fact, within limits always, the size and weight of the brain is but little correlated with the intellect of its owner. Even from the physical point of view, the larger part of the brain has little to do with the question, for it is the cortex, with the contained neurons, cells, axis-cylinders and prolongations, which really

counts, and there is no means of ascertaining what the variations of weight in the cortex are (it is perhaps usually about 37 per cent. of the brain weight, but this figure must have many variations) from the cranial capacity, or even from the brain itself, still less of enumerating the neurons which it contains or the connections which they possess. "How far are we, then," says Deniker, "from the true appreciation of cerebral work, with our rude weighings of an organ in which, with one part that would assuredly help us to the solution of the problem, we weigh at least three other parts having nothing, or almost nothing, to do with it? And even if we succeeded in finding the number, the weight, and the volume of the neurons, how are we to estimate the innumerable combinations of which they are capable? The problem appears almost insoluble."

Within limits, then, the size of the brain has little or no correlation with the intellectuality of its possessor. But within what wide limits. Let us take the lower limit and see how small a brain may be associated with average, or even more than average, abilities. I leave aside the question of the brains of small and pigmy races, though on that point it may be said that the Andamanese and other small races appear to have intelligences fully as great as those of other savage, uneducated and undeveloped races. But I will take another instance which will bear out the point which I wish to make—that the size of the brain is not an index to the intellectual capacities of its owner. Amongst the dwarfs who have been publicly exhibited

there was in existence some few years ago a little creature of the name of Paulina Musters, better known as "Princess Paulina." She was twelve inches in length at the time of her birth, and at her death—which took place in her nineteenth year—she measured but nineteen inches, being thus the size of an ordinary baby. Her usual weight was from seven and a half to nine pounds. She performed many feats when exhibiting herself, and, more than this, is described by the medical man who attended her in her last illness as being "of a good general education and speaking four languages—her native Dutch, French, German and a little English" The length of her head from chin to forehead was only five and a half inches, and though her brain was not examined after her death, one can imagine what a tiny structure it must have been when we consider that forty-five ounces is an average weight for a female brain and that her body only weighed from 120 to 144 ounces. Yet she was able to do with it as much or more than others do with brains probably three or four times as heavy. One other curious point in connection with this little woman must not pass unnoticed. All medical men know that children must be given smaller doses of medicine than adults; sometimes, in the case of powerful drugs, very much smaller doses. I suppose if one were to be asked why this is, one would say that it is because children are smaller and their bodies have not the resisting force inherent in those of larger size. When the doctor was called in to see Paulina he found it necessary to administer heart stimulants,

and he did what I am sure any other medical man would have done, prescribed them in children's doses. To his surprise he found that they did not act as they would have done in the case of a child, and he was obliged to administer the full adult dose in order to secure the effect which he desired. Here the body was as small or smaller than that of most children, so that we can only explain the facts by supposing that the resisting power was in the nervous system, and that that system had the adult powers though it fell so far short of the adult size. Powers of this kind, whatever they may be, cannot then be measured by the size of the organ with which they are associated. They belong to a region where weights and measures avail us nothing.

We have already seen that the shape of the skull, so far as the cephalic index is concerned, and the same is true—within limits—of the altitudinal index, has no bearing upon the intellectual powers. We may go further than this and say that extreme and unusual shapes of skull in otherwise normal and non-pathological persons do not interfere with the operations of the brain. Fashion has here dictated for us a series of experiments which no medical man would ever have dared to carry out. Amongst many races the fashion of modifying the shape of the head by keeping it wrapped in tight bandages during the period of growth has held sway, and the way of applying these bandages with the consequent distortion has been varied in several directions. In France, and more particularly in South America, among the indigenous

natives of that part of the world, strange shapes of head have been produced by these means. The Natchez Indians, the Aymaras, the Toltecs and the Chinchas have all adopted these means of modifying the head, and have produced, now an exceedingly flattened, now an unduly high, a grooved or a trilobed cranium. The custom was so prevalent as to have been actually forbidden by the Synod of Lima in 1585, and again interdicted by the Governor of the same place nearly two hundred years later. These actions were dictated by the supposed evil influences produced upon the health by this treatment of the head, and it is possible that it may have predisposed to some complaints, though there does not seem much evidence that it did so. But what effect had it upon the mental powers ? None, so far as we know from the records of the French examples, whilst so far as regards the American cases, Topinard, when considering this point, says that it is to be remarked that the races of America which had the least respect for their cranial form were those who, like the Aymaras and the Toltecs, had attained the maximum of prosperity in the new continent and have left the most marvellous monuments behind them.

Turning now to the question of the oldest known skulls, one is first confronted by the difficulty of coming to a conclusion as to the real age of any given example, of the time, that is—even, be it particularly observed, the *relative* time—at which its former owner lived upon the earth. For collocation of objects does not always mean

contemporaneity. To take an instance which will make my meaning clear, a murderer might bury the remains of his victim, unclothed and destitute of any accompanying objects, in a bank of glacial drift, perhaps even amongst the remains of the elephants of an early age. Years afterwards excavation might accidentally reveal the existence of the skeleton of the victim. Would it be right to regard it as having belonged to the date of the mammoth because the two sets of remains were found together? I have selected a not very likely instance, though a possible one, but any person can easily imagine for himself a score of different ways in which the bones of a man of very much later date might be found in relation of locality to objects of an earlier period. Hence the first thing that one has to do in the case of any given find of human remains, which might be assumed to belong to a remote period, is to make certain that it is to this period that they do belong, and not to one much later. As we shall see from some of the instances I shall bring forward, this is by no means an easy matter, and as a result there are very wide divergences of opinion even amongst those most competent to speak on such matters, on this very point of age. When Huxley wrote his *Man's Place in Nature*, in 1863, one of the two skulls to which he directed most of his attention was that found at Engis, in the valley of the Meuse, in Belgium. This skull, he assigned, on the high authority of Sir Charles Lyell, to the mammoth period. Yet according to Deniker, a writer of the present day, this skull only possibly belongs to the

Magdalenian period, which is much nearer our own time than the Mousterian or mammoth era, and no one would, I suppose, now claim that the Engis skull was one of the most ancient group. Some have even thought that it may be neolithic. Again, according to Topinard, Boule, and most other authorities, the remains found at Castelnedolo in Italy are purely an interment, as to the date of which no certain opinion can be formed, whilst Sergi still maintains that they may be the remains of tertiary man. It is obvious that it would be very dangerous to found any theory on data so uncertain as these, and I might go further and point to the Cro-Magnon, Cannstatt, Furfooz and Solutré skulls, all of which have at one time been assigned to a considerably earlier period than would now generally be allowed to them. As a matter of fact, out of forty-six skulls which have at one time or another been claimed to be quaternary, Deniker, one of the most recent authorities, only admits seven as certainly belonging to that period. Amongst those he does not even reckon the Neanderthal skull, over which so much ink has been shed. This celebrated skull was the second dealt with by Huxley in the book mentioned a few lines above, and for various reasons some space must be devoted to it, even though it be not now admitted by all to belong to the most ancient times. Some do still assign to it a very early date, Sergi, for example, going so far as to say that " it is definitely accepted that the Neanderthal skull is the most ancient witness to the appearance in Europe of man with well-defined osteological characters."

It is rather curious that the book from which this extract is taken should have appeared in the same series as that in which Deniker's book also appeared in the previous year (1900), where the statement occurs that "perhaps we should refer to this (the Mousterian) period the skulls which cannot be definitely traced to a certain alluvial bed, like those of Neanderthal, etc." But I shall show very shortly how wide have been the divergences of opinion in connection with this skull. It is an incomplete fragment of a skull, the vault or vertex has very prominent ridges above the eyes and a very small altitude. This skull was found in 1857 in a cave in the Neanderthal, near Dusseldorf, and on its first discovery was declared to be that of a being, if man at all, of a race inferior to any now existing upon the earth. But, by degrees, more careful examination led to the adoption of totally different opinions. Setting aside the statements of those who saw in it a pathological skull, and there were those who held that view, it soon became clear that it was a skull which in many respects resembled those of persons still in existence upon this earth, not to speak of others whose remains are left to us. Huxley declared that this skull was in no sense intermediate between the skulls of men and apes, and Sir William Turner showed that its characteristics are closely paralleled both by skulls of existing savage races and even by occasional specimens of modern European crania. Moreover, he claims that the large transverse parietal diameter—or breadth as we might put it—compensated for the brain

space lost by the retreating forehead and flattened occiput. Perhaps, however, Schwalbe's conspectus of the differences of opinion as to this skull, published in his paper of 1901, will, more eloquently than anything else, illustrate the conflict of opinion which has raged round it. And it may be added that other views as to this skull might have found a place in this conspectus. Here then is the list of opinions :

I.—It is no typical skull, but is a modified individual type.

1. The modification has occurred through too early union of the sutures which separate the individual bones of the skull from one another.—Barnard Davis.

2. The skull belonged to an idiot.—Blake, C. Vogt (at one time), Pruner-Bey (at one time), Hölder, Zittel.

3. The skull shows so many pathological changes, as indeed does the whole skeleton, that it cannot be taken as the type of a race. Virchow (1872), Ranke.

II.—The skull is one belonging to a still existing race.

1. It is quite a recent Kossack skull.—Mayer.

2. It belongs to a historic race.

(*a*) Old Kelt or German.—Pruner-Bey (at one time).

(*b*) Old Dutch or Frisian.—R. Wagner.

(*c*) Frisian.—Virchow (1876).

III.—The skull belongs to a primitive human race, but is connected by intermediate forms (neanderthaloid) with the lowest existing primitive races.

1. Shows similarity to Australians.—Huxley, Lyell, C. Vogt (at one time), Quatrefages and Hamy.

2. Belongs to the oldest palæontological dolichocephalic race, the Cannstatt race.*—Hamy, Quatrefages and Hamy.

IV.—It belongs to a race differing widely from existing races, the Neanderthal race.—Schaafhausen, Fraipont and de Lohest, de Mortillet, Sergi.

V.—The skull belongs to a form which differs specifically and *perhaps generically* from all recent human races.—King, Cope, Schwalbe. I need not comment upon the impossibility of founding any certain arguments on a skull concerning which scientific men have so much differed, and concerning which some of them have even maintained at different times wholly different theses.†

Yet, in the most recent text-book on Zoology,‡ I find the following statement: "Man is not known fossil till the Pleistocene. He is there represented by *H. sapiens*, and by an extinct species, *H. primigenius*, Schwalbe, (*neanderthalensis*) from the Neanderthal (1856), from Spy (1885), and from Krapina in Croatia (about 1899), and possibly

* According to Deniker, there are no data which make it possible to assign any date to the Cannstatt skull.

† See p. 191 for the latest views on this much-debated skull.

‡ By Adam Sedgwick, 1904.

from other localities. This extinct species is not thoroughly known, but it clearly belongs to a lower grade of organisation than *H. sapiens*." Whether, in the teeth of the extraordinary divergence of opinion, not merely as to relative date, but as to general characteristics, which the conspectus just given so clearly betrays, a dogmatic assertion of the kind just quoted is in any way justifiable, my readers can judge for themselves. Omitting all account of a number of skulls described in this article when it first appeared, since the more important examples will be dealt with at a later page, we may pass to the consideration of the remarkable remains discovered a few years ago in Java by Dr. Eugen Dubois. These finds were made in a probably Pliocene deposit, and consisted of (1) a molar tooth, (2) a fragment of skull found one metre distant from the tooth. These discoveries were made in 1891. In the following year were found (3) a femur at fifteen metres distance from the skull, and (4) a second molar tooth at three metres from the same. It is, of course, by no means certain that all these portions of bone belonged to the same body, nor can that question ever be definitely set at rest. It is also a curious fact, as the discoverer of the fragments pointed out, that during five years' work over an area some hundreds of square miles in extent, these were the only fragments of the kind discovered.* Concerning the actual nature of these remains there is the most extraordinary divergence of opinion, a divergence

* See p. 185 for further notes on the excavations and their result.

which renders it absolutely hopeless to count this skull in the building up of any theory, since opinions differing from one another *toto coelo* have been set forward by men of equally great distinction and equal right to come to a conclusion on the matter. I will leave aside the question of the femur; and with regard to the molars, which are unusually large, will only quote the opinion of A. Keith, that, from the size of the tooth, it is clear that the animal which possessed it must have had a long palate and very large temporal muscles, whilst the fragment of skull shows only very small indications of this muscle. I will turn to the fragment of skull itself, that being the subject with which we are now concerned, and will quote first of all Dubois' own summary of the opinions concerning it as given in his paper published by the Royal Dublin Society: "Whilst on the one side W. Krause, at the January meeting of the Berlin Anthropological Society, stated, as his opinion, that the skull-cap belonged without any doubt to a large ape, and on another occasion declared it to be that of a hylobates; whilst Waldeyer stated that the skull-cap might be attributed to a hylobatide, and again (at the Anthropological Congress of Cassel) that it could only have belonged to a higher form of anthropoid ape. Professor Cunningham, at a meeting of the Royal Dublin Society, regarded the cranium as undoubtedly human; and also Sir W. Turner and A. Keith considered it as a human remnant. Rudolf Martin is of the same opinion, and finds a total conformity in all real points with the human skulls of Neanderthal

and Spy. The reviewer in *Nature* considers it as that of a microcephalic idiot. And more recently Topinard declares the skull-cap to be human and Neanderthaloid. He considers *la question jugée*, whilst nearly at the same moment as his article appeared three other famous French anthropologists, MM. Hamy, Manouvrier and Verneau, declared, after having examined the skull itself, that it could not be human. At the Leiden Zoological Congress Virchow declared the Java skull to be an ape skull. In the opinion of Sir William Flower and Professor Marsh, who were present at the same session, it cannot be human, nor can it be regarded as that of a true ape." To this medley of opinions I will add the statements of two of the authorities quoted above. Keith says that the strong keeling along the metopic and anterior parts of the sagittal sutures (*i.e.*, the median longitudinal line of junction of the cranial vault) suggest that the bones found themselves in a position to expend a good deal more osseous matter after the brain had ceased to make any demand upon them, so that it would be necessary to examine similar specimens from the same locality—which, by the way, are not forthcoming—in order to obtain assurance that it was the skull of a normal individual. Sir William Turner, after pointing out the extreme difficulty that there is in arriving at even an approximate estimate of cubic capacity from so imperfect a fragment of skull, goes on to state that, accepting Dubois' estimate as correct—it is probably not an overstatement of the capacity—"three Australian

women were below it in capacity, and a considerable number were only a little more capacious," and other savage races have an equally small capacity. "It follows, therefore," he continues, "that a human cranium, smaller in its capacity than 1,100 cc. (the estimated cubic capacity of the Trinil specimen), is yet sufficiently large for the lodgement of a brain competent to discharge the duties demanded by the life of a savage." But the divergences of opinion are perhaps best shown by figures prepared by M. de Mortillet, and tabulated by him in a book published some little time ago, as to the opinions which had up to that date been expressed respecting the fragments discovered at Trinil. Here are the opinions as given by twenty-one observers.

The Remains.	Belong to Man.	To an intermediate type.	To a monkey.
2nd Molar	— ...	7 ...	... 2
3rd ,, ...	4 ...	8 ..	... 6
Femur	13 ...	6 ...	... 1
Skull	6 ...	8 ...	... 6

What do we learn from these and other early skulls? In the first place, that there has been, and indeed still is, a doubt and uncertainty about some of them, not only as regards their nature, but as regards their date. We learn also that these doubts and uncertainties are entertained by leading authorities on the subject, a fact which renders it absolutely impossible to utilize, at least at present, some of these skulls for the foundation of a firm edifice of argument. Then, in the next place, we learn of those skulls whose date and character are least in doubt that they are clearly human

skulls, closely resembling in all salient features the skulls of men and women living at this day, and in no sort of sense intermediate between men and apes.

Huxley, in 1863, admitted that "the fossil remains of man hitherto discovered do not seem to me to take us appreciably nearer to that lower pithecoid form, by the modification of which he has, probably, become what he is."

Twenty-six years later, in 1889, Virchow,* one of the greatest anthropologists and men of science of the last century, was summing up—with the evidence of all the skulls mentioned in this paper, except that of Trinil, before him—the question of the origin of man's body. What was his verdict? Speaking at the twentieth general meeting of the German Anthropological Association, at Vienna, he alluded to the first meeting which the society has held. At that time, he says, "no one doubted that the proof would be forthcoming, demonstrating that man descended from a monkey, and that this descent from a monkey, or at least from some kind of animal, would soon be established. This was a challenge which was made and successfully defended in the first battle. Everybody knew all about it and was interested in it. Some spoke for it; some against it. It was considered the greatest question of anthropology. Let me remind you,

* It is now the fashion of some writers to state that Virchow's "vast knowledge and range of thought have been somewhat neutralised by his excessive conservatism." But this is only a roundabout way of saying that Virchow's views do not agree with those of the writers in question, and that Virchow's opinion in consequence is not worthy of being taken into consideration.

however, at this point," he proceeds, "that natural science, so long as it remains such, works only with real, existing objects. A hypothesis may be discussed, but its significance can only be established by producing actual proofs in its favour, either by experiments or direct observations. This Darwinism has not succeeded in doing. In vain have its adherents sought for connecting links which should connect man with the monkey. Not a single one has been found. The so-called *pro-anthropos*, which is supposed to represent this connecting link, has not as yet appeared. No real scientist claims to have seen him. Hence the *pro-anthropos* is not at present an object of discussion for an anthropologist. Some may be able to see him in their dreams, but when awake they will not be able to say they have met him. Even the hope of a future discovery of this *pro-anthropos* is highly improbable; for we are not living in a dream, or in an ideal world, but in a real one." Three years later the same great authority maintained the same position, saying of development: "And so, in the case of man, we are repulsed all along the line. All the researches undertaken with the object of finding continuity in progressive development have been without result; the *pro-anthropos* does not exist; the man ape does not exist; the missing link remains a phantom." Again I would point out that at the time this was written Virchow had before him all the evidence which we have to-day,* except that of the Trinil

* *i.e.*, 1905, the date of this article.

skull. This skull he subsequently saw and proclaimed to be that of an ape; and he would be a bold man, in the face of the divergent opinions which I have recapitulated above in connection with that much-debated skull, who would maintain that it had made much difference to the controversy. It is not even counted in the matter in Sedgwick's Zoology, from which quotations have been made above. In fact, so obvious is it that the skulls of these ancient races—be the exact date of their habitation of this world what it may—are nothing more or less than the skulls of human beings, that Kollmann, the distinguished Swiss anatomist, has proclaimed that man is a *Dauertypus* or "persistent type," which has not varied since quaternary times, those, by the way, being the first times when we come in contact with anything like human remains or, indeed, with any certainty, in contact with any implements which can be legitimately considered as of human manufacture. To pursue this point further would lead me in directions other than those which I have marked out for this paper, though the obvious suggestions which arise are well worthy of consideration and discussion. I will sum up what I have tried to lay down by saying that science can only deal with facts, and that all theories which are not fully supported and buttressed by solid evidence may be fascinating, inspiring, probable, what you please, but that they remain only theories and nothing else. This seems a simple elementary statement, but it is one which is too often forgotten by a certain class of writers.

Well, in respect of one particular theory I have tried in these pages to sift the evidence and give the views of the most credible and recognised authorities in connection with one line of argument alone—it must be admitted, however, a very important line of argument.

I have carefully limited myself to this single point, and must not be taken as making any statements to the right or to the left of it when I say that, so far as the craniological evidence goes, those who desire to prove the evolution of man's body from that of a lower form have completely failed to make out their case.

VII: THE EARLIEST MEN

QUESTIONS respecting the earliest human inhabitants of the earth are not merely engaging the best attention of the learned, but are seriously occupying the thoughts, and sometimes, it would appear, grievously disturbing the minds of those who, without laying any claim to the title of learned, extend their reading beyond the limits of current fiction. No one indeed can read the newspapers with any care, without from time to time as some new discovery is made, having questions of the kind indicated forced upon his attention. There is nothing wonderful in all this, indeed the wonder would be if our attention were not attracted by such questions, so closely related to ourselves and to matters which many of us hold dear and which appear—it is only in appearance, but it seems real to those imperfectly acquainted with the facts—which appear, I repeat, to conflict with those teachings of religions which we so profoundly respect.

How long ago is it since man first appeared on this earth? What sort of a person was this far-off ancestor? Did he resemble ourselves, or was he like any of the other races of human beings with whom we are familiar? Or was he a creature whom we should never recognize as a man and a brother if we were able, like Peter Ibbetson and the Duchess of Towers in the story, to dream ourselves

backwards until we could in vision behold those far-off days? How did this individual live? What did he make? Had he any ideas about art? About God? About another life? All these and many other questions are constantly being asked, and what is most strange, are being asked not without considerable prospect of an answer being returned, and that answer one which, up to a point, we may quite reasonably accept in spite of the remote and shadowy period to which it applies. To sketch very briefly the replies to some of these questions, and to indicate as far as possible the point at which reasonable certainty ceases and surmise—sometimes legitimate, sometimes wholly visionary—commences, is the object of this article. It is written in order that Catholics—who require such knowledge certainly not less urgently than other people—may know exactly what is established fact and what is mere surmise, what, in other words may, nay must, be believed, and what may be rejected or accepted, according as the wavering balance is inclined this way or the other by fresh pieces of information coming to light.

Before attacking the questions indicated, indeed as an essential preliminary to any such attack upon them, it will be necessary to clear our minds as to the fundamentals of chronology, for on an easily understood misconception of those fundamentals depends a great deal of the confusion and, further, of the unsettlement of mind which exists on these questions.

We may say, then, that there are: Geological Time, Archæological Time, and Historical Time.

And, we may add, that it is important that these three should not be confused with one another. Let us briefly consider each of them. *Historical Time* is the chronology of recent events capable of being set down in actual, definite, incontrovertible (usually) figures. Thus the Battle of Senlac took place in A.D. 1066, and the Declaration of Independence on July 4th, 1776. With facts of this kind we have nothing to do in this paper, but with certain chronological problems, and notably with that of the so-called Ussherian chronology we shall find ourselves concerned in a later section. Meantime it may not be without interest to consider how far back we can safely go in actual historical chronology. As far as records go, Egypt and Babylonia afford us the best chance, and of these Egypt is perhaps the better known example. Now in the history of that country, we can tread with security as far back as the conquest of Alexander (B.C. 332). But that period, need it be said? is only as yesterday in the long history of this earth, or even of the history of man upon it. From Alexander backwards to the commencement of what is known as the First Dynasty, our path becomes less certain. There is a kind of a chronology, but how uncertain and indefinite that is may be gathered from the fact that the dates assigned for the commencement of the First Dynasty vary from B.C. 3315 to B.C. 5510, and that one of the leading authorities (Petrie), who in 1894 fixed the date as 4777, has felt himself compelled by further evidence to change his opinion, and assign B.C. 5510 as the proper date (in 1906).

Yet even this period is only as the day before yesterday in the history of the globe, or even of its human inhabitants. So that we may safely say of Historical Time, that of the events of to-day and of yesterday we are tolerably sure, and as to those of the day before yesterday we can make reasonable guesses. Of those of the days before that we *know* nothing, though we can (and do) make many surmises as to them.

Archæological Time, which we may define, for our present purposes, as commencing with that uncertain epoch when man first made his appearance on this world, merges into Historical Time on the one hand, and like Historical Time, is, of course, co-existent with Geological Time. In part, in very large part, and at its earlier periods almost entirely as we shall see, Archæological Time depends upon Geological Time for its estimation. For it is by the stratigraphical character of early deposits almost alone that we are able to arrive at any conclusion, not merely as to their actual, but even as to their relative chronological positions. We are quite safe at present in assigning certain *Periods* to Archæological Time, and more or less safe in assigning certain objects to them. Thus there was a time in every part of the world when mankind had no knowledge of the use of metals, a time which we call the Stone Age. But this time was by no manner of means synchronous in all parts of the world. It is many a long year since Europe emerged from this stage of its development: it is only the other day that savage tribes in remote parts learnt that there were other

implements than those they constructed from sticks and bones and stones.

This Stone Age may, in many if not most parts of the world, be divided, roughly enough, into two periods : an earlier or Palæolithic, and a later or Neolithic, according to the character of the implements made. The former may have been preceded by an Eolithic Period ; it merged into the latter, in some places at least, by a Mesolithic Period. At any rate at the end of the Stone Period man came into the knowledge of how to smelt and use metals, and the Metallic Age commenced. After a brief Copper (or Æneolithic) Period, which seems to have existed in many if not in most places, a great manufacture of bronze, which is an alloy of copper (ninety per cent) and tin (ten per cent.) came into being, and this is the characteristic material out of which implements were made in the age named after it—the Bronze Age.

As far as our present knowledge teaches us, and it is in the last degree improbable that facts will ever arise to disturb the conclusion, every race on this earth has at some time or another passed through a Stone Age, an era or phase of their civilization during which they were unacquainted with the use of metals. It is a little ambiguous to use the term " Stone Age," since that would seem to imply that no implements other than those made of stone were in use. Of course this is not the case, for man availed himself of shell, horn, and wood, as well as stone at this period. If we think of it as a non-metallic age, we shall clarify our conception.

After having passed through this stage of

development most races arrived, by means which cannot here be discussed, at a knowledge of the use of metals, and in some, perhaps in many instances, the metal of which they made discovery was copper, and of copper, at any rate in certain parts of the world, we find the earliest metallic implements made. But the manufacture of stone implements did not suddenly come to an end: it went on side by side with the limited copper industry. To this period of transition, when there was an overlap between the two forms of material, is given the name of the Æneolithic Period. This Period, wherever it occurred, was probably one of short duration, for it was soon discovered that the mixture of ten per cent. of tin with the copper produced a much harder and more useful metal, the mixture which we call bronze. It is possible that metal first came under the notice of some nations in the shape of bronze, that being brought to their knowledge by travellers, and this would account for the fact that there was no Copper Period in that particular area.

Throughout Europe, though not, as was the case with stone, throughout the world, everywhere there has been a Bronze Age preceding the discovery of iron, the dominant metal of the age in which we ourselves live. We have seen that a race might have escaped a Copper Age by having the more perfect metal bronze introduced to them by travellers, whilst they were still in their Stone Age. In the same manner within historic times tribes have been discovered, unacquainted with the use of metal—still in the Stone Age—to whom metal,

in the shape of iron, has been presented by travellers. Such races—the native Australians for example—have never been through a Bronze Age. The Bronze Age was not, therefore, universal throughout the world, nor was it synchronous in those countries in which it is known to have existed. It probably commenced in Europe some four thousand years ago, and lasted for something like two thousand years. But in Mexico and Peru the native populations, up to a comparatively recent time, were still in the Bronze Age.

What one has to remember about all these archæological epochs is, that they are not to be looked upon as much as periods of time, but as successive reaches in a river of progress, arrived at sooner in some cases, later in others. Perhaps the following table which summarizes what has been said, will assist the reader :

ARCHÆOLOGICAL PERIODS.

Stone or Non-Metallic Age.
- Eolithic Period (?)
- Palæolithic Period.
- Neolithic Period.

Age of Transition from Stone to Metals.
- Æneolithic or Copper Period.

Metallic Age.
- Bronze Period.
- Iron Period.

With most of these periods we are not concerned in these pages, but of the first two much remains to be discussed. This, however, must be deferred to a later point.

We have now to turn to the subject of *Geological Time*, with which a portion of our subject is inseparably bound up. Indeed, as some writer has very aptly remarked, the problem of early man is far more a problem of geology than a problem of biology.

Here again it has to be remarked that the use of the word "time" in connection with geology, is more than a little misleading. As in the case of archæology, "time" in the sense of a definite number of years can, as we shall shortly see, be applied only in the most tentative manner to geological epochs. We can, with more or less certainty, divide up the history of the world into geological eras, and, though not always with complete certainty, assign a given rock bed to its appropriate era, but when we come to attempt any method of dating, in terms of years, the time when this era was in being, we find ourselves confronted with a hitherto insuperable problem.

Stratigraphically, however, we may divide Geological "Time" into Primary or Palæozoic Secondary or Mesozoic; Tertiary or Cainozoic, and Quaternary or Post-Tertiary. With the first two of these, comprising vast areas of rock, and formed during vast, almost inconceivable ages of the world's history, we have nothing to do in these notes, since man is not directly connected with them. But in order to follow what has yet to come, it is necessary to deal somewhat more particularly with the two latest of these periods, and proceeding from the earlier to the later, we may set down the following classification :

LATER GEOLOGICAL PERIODS.

Tertiary.

Eocene. Here there is no question of man.

Oligocene. } (Wanting in Britain). At the end of this Thenay flints; and at the beginning of Miocene Aurillac flints.
Miocene. }

Pliocene. At the upper part of this, or at the lower part of the next, is the Red or Norwich Crag, associated with the rostro-carinate forms.

Quaternary.

Pleistocene or Glacial. Recent, which brings us down to the present moment.

Incidentally some points in connection with these periods will arise in our consideration of the relics of early man. There is only one point which must be dealt with here, and that is the question of the Glacial Period. This is a matter which is more fully discussed in the next article. Here it need only be said that it was a period during which large tracts of the world now occupied by mankind were rendered uninhabitable by glaciers of enormous magnitude and other accompaniments of an Arctic climate.

If we try to make any estimate of the actual number of years required for the events associated with the Glacial Period we shall find ourselves involved in immense difficulties.

In books of history, we find it set down that William the Conqueror reigned from A.D. 1066 to 1087. Less sharp-cut, but sufficiently definite,

is the statement in books on architecture that the Early English period of Gothic belonged to the thirteenth, and the Decorated to the fourteenth century. It would be exceedingly convenient and enlightening if we were able to say that the fourth ice age extended from B.C. —— to B.C. ——, or even to say that it covered so many years, and was approximately so many years ago. It is not the fault of geologists that this cannot be done, or has not been done. Numerous have the efforts been to solve the question, and equally diverse the answers made to the riddle. I shall not here refer to the very varying views which have been expressed by scientific authorities as to the age of the world, but will content myself by saying a few words as to the so-called "geological clocks." What is wanted to solve the question under consideration, is some kind of standard of comparison between past and present processes. For instance, no one now doubts that—with exceptions which need not here be considered—the processes which have shaped the world as it now is, are the same kind of processes which are shaping the world into what it will be in ages yet to come. If we could accurately measure the result of one of these processes to-day, and then compare it with the result of one of a similar process in the past, we surely ought to be able to estimate the amount of time which it would take that process to bring about that result.

In other words, we should have a "geological clock." Before mentioning a few instances of these, it may be as well to point out that a clock is useless as a measure of time, unless it is invariable in its

operations, unless in fact, as we put it, "it keeps time." That is just what none of the "geological* clocks" do. To leave the metaphor, we can never feel sure that the conditions of the process in the past have been identical with, or even very similar to, those which we have been concerning ourselves with in the present. In fact we can feel pretty certain that they were anything but identical. But if they were not identical, our "clock" which is not, like chronometers, "compensated" for all sorts of conditions under which it may find itself, must necessarily fail to "keep time," and this must necessarily deceive us if we place our confidence in it. Let us examine one or two cases in order to understand this somewhat important matter more fully.

There is, first of all, the matter of erosion by rivers. It is quite clear that river erosion has been going on for a good many years, and it is equally clear that it is going on at the present day; can we not measure the annual amount of erosion now taking place, and from the amount which has been eroded in the past, which can often be ascertained without much danger of error, form some conclusion as to the length of time which has elapsed since the river began its work, and so of the various deposits associated with it? This apparently simple calculation is vitiated by two things. In the first place, it is by no manner of means easy to gauge the annual amount of erosion, a fact which is abundantly proved by the very

* It is possible that an exception should be made in respect of the laminated Scandinavian sands, for which see p. 217.

different estimates arrived at in selected cases by different observers. Thus, for example, Sir Charles Lyell, a great authority in geological matters, estimated that the amount of time required for the erosion of a certain stretch of the Niagara Gorge was forty thousand years, an estimate, of course, based upon his calculation as to the annual amount of erosion. Yet, in 1907, C. K. Gilbert stated, in the publications of the United States Geological Survey, that in his opinion the amount of time required for the piece of work in question would be no more than seven thousand six hundred years.* In fact, with all respect be it said, these estimates are and must, so it would appear, always be, guesses—guesses made by men more likely to guess right than wrong perhaps, but, in the end, only guesses, and thus very far removed from being scientific facts as the more enthusiastic papers and magazines are prone to represent them. But there is yet another source of fallacy, and it is this : The conditions cannot be shown to be constant, nay more, everything points to the fact that they have been extremely inconstant during past ages. But this state of affairs would wholly upset the accuracy of our clock, and render any calculations based upon its record wholly fallacious.

In a very interesting little book on ancient human remains,† Professor Keith claims that the Thames is a reliable clock. "The Thames itself," he says (p. 22), "is to be our clock—one which has never ceased to mark time and record history

* See p. 220 for further account of this matter.

† *Ancient Types of Man.*

on its banks and valley." No doubt, but are we quite clear that it has always "kept time," even if we are quite clear that we know that "time?" Professor Keith makes the "provisional estimate" that subsidence has taken place at the rate of one foot per thousand years, and apparently that this rate has been a constant one. Thus he is able to date his skeletons at ten, twenty, thirty thousand years ago. But in this calculation all reference is omitted to the very differing physical conditions which must have existed during the long space of years which has rolled by since the Thames began its work. Nothing seems clearer than the fact that during quite a considerable part of that era, the volume of water discharged must have been enormously greater than that which has flowed under London Bridge since that was built. And this greater volume, of course, would mean a much more rapid erosion. Which, in its turn, would wholly upset the calculation as to time based upon it.*

It is quite clear that estimates of time of this character, however picturesque they may be, and however seductive to the journalist in search of a sensation, are quite useless and not to be depended upon. The same story applies to other geological

* That the calculation in question which would place the man of Galley Hill at 200,000 years ago, and those of Neanderthal at 500,000 to 1,500,000 years ago, are not acceptable to other workers, is obvious from the criticism of the work in question by M. M. Boule in *L'Anthropologie* (vol. xxiii, p. 218), in which, after speaking of the author as "peu familiarisé certainement avec les questions de géologie et d'archéologie préhistorique," he goes on to quote his estimates of age, and speak of their "imprudente hardiesse."

clocks. Take the case of those glaciers which have been in operation during the Glacial Period, and are still in being. De Mortillet selected these as his "clock," and based his calculations on the length of time which, as he calculated, it would have taken the Rhone glacier to deposit its terminal moraine, namely forty thousand years. But it is quite clear that the Swiss glaciers are comparatively trifling objects to what they must have been during the Great Ice Age. Dom Izzard* points out that

"glaciers of the Glacial Period cannot be compared to their degenerate descendants now remaining in the Alps, but rather with the glaciers of Alaska and the Himalayas. In a recent official communication of the geological survey of India, it is attested that in 1903 the glacier of Hassanabad extended itself in two months a distance of nine thousand six hundred metres. The Rhone glacier during the glacial epoch would be a vast mass of ice similar to this, and if it had advanced towards Lyons at the pace of the Himalayan glacier, it would have covered the distance in thirty years. The Alaskan glaciers do not furnish us with any rapid progress such as this, but they move much more rapidly than the present Alpine glaciers, and in addition the rate of neighbouring glaciers at the same period seems very variable. We cannot, therefore, take the present day Alpine glaciers as standards for movement in the glacial epoch."

* In a very interesting and useful paper in *The Oscotian*, 1913.

Finally, there is the question of the deposition of stalagmite, to which allusion may be made. When an inscription—as to the genuineness of which no doubt seems to be entertained—was discovered in Kent's Cave near Torquay, with the date 1688, and with a thin coat of stalagmite over it, it seemed as if we might get some ratio as to the time which must have been taken in forming the vast floors of stalagmite investigated in that cavern beneath which were objects of human manufacture. Yet observations in another cave after a great flood which had lifted the floors, brought to light ginger-beer bottles under a layer of stalagmite a foot in thickness. The fact is that the rate of deposition of stalagmite depends upon a whole range of factors, such as the amount of moisture and the quantity of dissolved carbonic acid which it contains, and no estimate of time can be founded upon it with any sort of security.

The above remarks may seem rather too prolonged, but the matter has purposely been dealt with in some detail, and for this reason : The vast epochs of time assigned for the existence of the human race upon the earth, are based upon calculations of the character of those just dealt with. Little wonder is it that there are such extraordinary discrepancies between the findings as to time of one writer and of another. The important point to bear in mind is that, whether long or short, the chronologies of geologists are all more or less of the nature of guesses founded on guesses, and as such liable to revision, and possibly to complete alteration as fresh facts come to light.

MAN AND HIS IMPLEMENTS.

As far as our present knowledge, we know of the existence of man upon this earth by the implements which he made before we know of him by his physical remains. The question bristles with difficulties as we have yet to see, but, on the whole, the statement just made may be taken to represent our knowledge at the moment. Nor is this difficult to explain. In his earliest days man no doubt used any object which came to his hand, stick, stone or shell, provided that it was capable of doing the bit of work which he had in hand at the time, whether that bit of work was the slaying of an animal, the preparation of its skin for wearing purposes or what not. Some of the implements thus employed would be perishable, and have long since disappeared, others—those of stone—would be practically imperishable, and these are what have come down to us as the earliest relics, though we have also objects of bone and horn of great antiquity. Much dispute exists as to which are the earliest objects which are quite clearly the work of man's hands, and it may be well to explain very briefly why this should be so. The first point to bear in mind is, that the utilitarian ideas of early man would very naturally lead him to use a natural piece of stone, where such would serve his purpose without any shaping or alteration. It is obvious that it must always be very difficult, and usually quite impossible, to detect the fact that a given fragment of stone was once an implement used by man when that fragment has not

been obviously shaped by intention for some purpose. Let us proceed a stage further, and suppose man shaping his stones so as to become somewhat more serviceable implements than natural pebble or flake. It can hardly be doubted that these first attempts would be so exceedingly like the results of nature's own operations as to render it a very difficult, perhaps an impossible, task to decide whether a given object had been produced by the one agency or the other. Then, however, there comes a stage when the evidence of workmanship becomes clearer, and in the mind of the expert no sort of doubt is left that the rude fragment of stone which he is examining has been purposely fabricated by the hand of man. Yet even here, when these discoveries were first made, the scientific world was exceedingly incredulous. It is not necessary here to detail how Boucher des Perthes, in the middle of the last century, made his classical discoveries of palæolithic implements at Abbeville in France, of the controversy which arose as to the nature of these objects, and of their final acceptance by all men of science. It is as well to bear this history in mind when one is considering other and still unsettled controversies with regard to so-called implements.

After these preliminary remarks, we may now turn our attention to some of the fragments of stone which have been claimed as the work of man's hands. In 1867 the Abbé Bourgeois discovered at Thenay, near Orleans, broken flints which he believed to be implements of human manufacture. These were in beds of the Upper Oligocene Period,

and since no signs of human remains had or have been found of that geological date, de Mortillet, who was convinced of the human character of the implements, postulated a semi-human precursor of man as their manufacturer, and named him *Homosimius bourgeoisii*. It may be added that no trace of this imaginary creature has ever been discovered, and that the flints themselves are now believed to have been of natural origin, *i.e.*, not shaped by the hands of man, but by natural causes, such as water, earth-pressure, lightning perhaps, and so on. A similar statement may be made as to the Puy Courny flints described by J. B. Rames in 1877, and found by him in Upper Miocene beds in Auvergne. De Mortillet again postulated a hypothetical *Homosimius Ramesii* (who has never materialized) as their maker.

Omitting other less important cases, we may come to the question of the so-called "eoliths," as to which so much controversy has been carried on during the past twenty years or more. These objects, which have been found in considerable quantities in England and on the Continent, are undoubtedly of great antiquity, though their exact geological position is not certain. Up to a comparatively recent period, there was a strong body of opinion favourable to their artificial nature, but the most recent observations have rendered their character much more doubtful. That such implements may be formed by cart wheels from the flints newly laid upon a road, proves but little, for after all that is a form of work by man, though unintentional, and is not strictly

comparable to the operations of nature. Something similar may perhaps be said as to the discovery that "eoliths" can be and are produced by the revolutions of an iron rake, in a mixture of water and chalk (containing flints) and clay, in the process of cement making as practised near Mantes.

But the most crushing piece of evidence is that brought forward by the learned Abbé Breuil,* who has found "eoliths" in Lower Eocene sands in Clermont, with the detached flakes in situ, showing how the process has taken place. He has proved conclusively that these so-called implements can be made by one process of nature, and that a process which must have been in operation during long ages, and even at this present moment, and that process is the gradual movements of strata whilst settling down under pressure of the soil. This pressure causes the flint nodules to be squeezed against one another, and thus flakes to be detached which eventuate in the "eolith." Now it may be taken for granted that no implement or so-called implement can be accepted as unquestionably the work of man's hands, unless it it quite clear that it cannot owe its shape to any other cause. It has been shown that "eoliths" *can* be produced by purely natural means. Therefore it cannot be shown that any of them were the works of the hands of man. Nevertheless the fact remains that we ought to expect to find something much more rude than the comparatively finished implements which have yet to be dealt with. The

* *L'Anthropologie*, 1910, vol. xxi, p. 385.

only question is whether we shall be able—when such finds are submitted to us—to say, with the slightest certainty, whether they were made by man or not.

We may pass from these to the Icenian or rostro-carinate implements, found by Mr. Moir below the base of the Red Crag of Suffolk, and described very carefully by Sir Ray Lankester.* The geological period to which the Red Crag belongs is not quite clear. It has usually been assigned to the Pliocene series, but Sir Ray thinks that this is an error; that its fauna proves that it should be included in the Pleistocene Age. At any rate it is of great antiquity, and, if the objects described be really of human manufacture, a point on which Professor Sollas has recently thrown doubt, they unquestionably set back the date of man's appearance on the earth to a very distant date. The position of these flints must at present be left undecided until the controversy has developed, and further facts—as in the case of the "eoliths"—appear, when it is possible that a definite opinion, one way or another, may be capable of expression.

De Mortillet divided the Palæolithic epoch into four periods, each associated with a special culture or form of implement. This scheme has been somewhat enlarged by other workers, and Sollas' modification may here be given, as it will serve as a convenient scheme when dealing with human remains in the later portion of this paper.

Commencing with the earlier and proceeding to the later stages we have

* *Philosophical Transactions*, pp. 202, 283.

Mesvinian.	
Strepyan.	*Lower Palæolithic.*
Chellean.	
Acheulean.	
Mousterian.	*Middle Palæolithic.*
Aurignacian.	
Solutrian.	*Upper Palæolithic.*
Magdalenian.*	

In connection with this it may be added that the Mesvinian and Strepyan Periods are as yet not fully accepted by all authorities of prehistoric archæology; the "implements" belonging to them may or they may not be the work of man's hands. But with the Chellean, at any rate, we enter a region where there is no doubt, nor of course is there any regarding any of the later subdivisions of the Palæolithic Age. After this has passed away, the Neolithic Period is entered upon, as stated at an earlier period of this paper. The difference between the kinds of implements found in the various stages of the Palæolithic Age are very marked and very interesting, but, from the point of view of this paper, it will not be necessary to deal with them here. We mark the fact that there are such differences, and that they are quite recognizable, and pass on.

THE REMAINS OF MAN HIMSELF.

Before proceeding to describe and comment on the examples of early human skeletons or portions

* De Mortillet's four periods were: Chellean from Chelles, a few miles east of Paris; Mousterian from the cave of Moustier on the river Vézère, Dordogne; Solutrian from the cave at Solutré near Maçon, and Magdalenian from the rock-shelter of La Madeleine, Dordogne.

thereof, it will be convenient to deal with a few general points in connection with this section of our subject. In the first place, then, it may be said that the remains of early man so far discovered are but few, and that it is not to be expected that they will ever be very numerous. Only under the most favourable and unusual conditions could they have been preserved to the present day, and even when they have been preserved to the present day, it is often (one may say invariably with regard to the very earliest cases) in a much mutilated condition. Nor is this difficult to understand, as we shall see if we consider the condition and surroundings of early man and in connection therewith some of the difficulties which arise when we come to attempt to place his skeletal remains in their appropriate geological or cultural horizon. When early man came to die his tribe might either feed on his remains or leave them to lie where they were at the time of his death, or they might inter them with or without cremating the body. Cremation we may dispose of at once, for, though it was a favourite practice in a later period of the prehistoric age, we have no evidence of it during those earlier stages with which alone we are here concerned.

Let us suppose that his remains were left to lie where they were when life fled from the body. The flesh would gradually disappear, either devoured by wild beasts—of which there were great numbers and varieties—or disposed of by ordinary process of decomposition. The bones might for some considerable time resist disintegration, but eventually they, too, in the course of long ages,

would disappear, unless some lucky accident occurred to preserve them or some portion of them. They might be covered up, or the complete body might be covered up by wind-blown sand, by gravel or earth brought down by a flood, by a land-slip or by other natural, fortuitous circumstance. Then long ages afterwards, the gravel-seeker or some other son of toil gets to work and exposes the remains. Let us suppose, however, that his fellows resolve to bury their dead comrade. They may do so by depositing his body in a cave, as was actually done in many portions of the Palæolithic Age, though not, as far as we are aware, in its very earliest stages, and if that cave was rendered inaccessible to wild beasts, the remains would have the best chance of surviving to our own day.

Incidentally it may be added that where primitive man took the trouble to place the remains of his dead brother in security, he in the vast majority of cases placed with them some of the implements which the dead man had been in the habit of using whilst on earth. These offerings are called "grave-goods" or "accompanying gifts," and they are important from two points of view. First of all they throw great light upon the period to which the remains belong. Thus, if a bronze dagger is found with a skeleton, in an untouched interment, it must be clear to everyone that the remains are not *earlier* than the Bronze Age. They may be later, because the implement may be one of an earlier period, placed with the remains of the dead man for some ritual or other reason, but

they cannot be earlier. And so with the various kinds of stone implements, when they are found with the remains of the dead, they are of great assistance in enabling us to say at what stage of the world's history he lived.

But there is a further point of perhaps even greater interest in connection with these "grave-goods," and it is this. All over the world, and at all stages of the world's history with which we are acquainted, these "grave-goods" have one significance and one only, and it would be illogical and absurd to deny that the same significance does not attach to them in the period before history began. These offerings were placed with the dead body, because it was believed that the man did not all die, but that something of him remained which went to live in some other existence—perhaps very similar to that enjoyed by the dead man when on earth—in which he would need the implements which were placed by his dead body. Hence wherever these "grave-goods" are found, we may conclude that those who placed them there believed in the existence of what we call the "soul"—we do not know how they spoke of it or thought of it—of the man himself, as apart from his body, in some other world invisible to his fellows. To dispose of this part of the matter at once it may here be said that the earliest race of whose burials we have any knowledge—as will appear at a later stage—is that known as the Mousterian. A complete account of an interment of this period in a cave known as La Chapelle aux Saints in the Dordogne district, was given by

MM. les Abbés A. and J. Bouyssonie et L. *Bardon.**

Around this body lay a great number of well-made implements of the period, and bits of the red ochre with which we may reasonably conclude that the members of the tribe, like other savages, were in the habit of decorating their bodies. Further, bones were placed over the head, in fact, as Sollas says, "this was evidently a ceremonial interment, accompanied by offerings of food and implements for the use of the deceased in the spirit world." And he continues: "It is almost with a shock of surprise that we discover this well-known custom, and all that it implies, already in existence during the last episode of the Great Ice Age."

After this digression we may return to the question of interment which may not—in an overwhelming number of cases could not—have taken place in a cave. Then the survivors must have (a) dug a hole in the ground, or (b) in the side of a bank, or (c) have heaped up a pile of earth or stones or both—a cairn in fact—over the remains. It will be observed that a similar result, so far as the remains are concerned, might occur from natural causes, and the first thing which has to be determined when bones of an early man are in question, is whether they were interred or not, and this is by no means always a problem easy of solution.

* *L'Anthropologie*, 1908, p. 513. Perhaps one may be allowed to call attention here to the extraordinary number of facts in connection with prehistoric archæology which have been brought to light by Catholic clerics, *e.g.*, Breuil, McEnery, and those mentioned above *cum multis aliis*.

In the first place the discovery of these remains must necessarily, in almost every conceivable case —except where caves are being purposely searched for the remains—be made by some labourer wholly ignorant of the matter in question. If the remains themselves are not destroyed or grievously mutilated, the surrounding conditions must necessarily have been so much interfered with as to render it very difficult, perhaps almost impossible, to say whether the body lies in disturbed or undisturbed earth, that is, whether we have to do with an interment or a natural position of the body.

The next question refers to the objects found with the remains. If it is an undoubted interment and remains are found with it, as in the case of La Chapelle aux Saints, no question arises. But let us take the case of a fragment of skull found in a gravel pit in association with palæolithic implements, and the teeth of elephants and a hippopotamus. They *may* all belong to the same period it is true, but then, on the other hand, they may not, for they may belong to different periods, and have been rolled together in the same pit by some great flood. Here it may be remarked that animal remains, particularly of the kind alluded to above, are of great service in assigning a period to things found with them, but only where it can be definitely proved that the collocation of the two classes of objects is not wholly accidental. A fragment or the entire of a skull of a Roman soldier might quite conceivably be found in gravels containing palæolithic implements and teeth of the kind alluded to above, but long ages would

have separated the various things under consideration.

It is well to bear in mind that there are always three questions to be asked in connection with any discovery of human remains; that the reply to any or all of them is often most difficult and most doubtful, yet until these questions are answered, no absolutely certain decision as to the precise scientific value of the discovery can be arrived at. These questions are:

First: What is the geological period of the stratum in which they are found.

Second: Do the remains in question belong to that period, or were they of later date, and introduced by man or by other means into a stratum with which they were not related by time?

Third: Were any implements or bones or teeth or other such objects found with the remains, and, if so, was the collocation accidental or was it significant?

Having cleared away these preliminary points, we may now proceed to a brief consideration of the chief remains of early man at present under discussion.

EARLY HUMAN REMAINS.

Very briefly, from considerations of space, must the chief examples be touched upon, and in so doing an effort will be made to avoid details, and to give the main features of interest to the general reader.

*The Trinil Remains.** Discovered in Java by Dubois in 1891. They consist of the top part of a skull, two molar teeth, and a thigh bone found in the same locality, but forty-six feet apart. They were clearly not an interment, and the first difficulty which arises with regard to them is whether they all belonged to the same individual or not, a difficulty which can perhaps never be set at rest. An attempt to throw light upon this and other disputed points was made by the expedition of Mdme. Selenka, the results of which have been recently published. After enormous labours nothing was found with the exception of another tooth, pretty certainly human. So far as can be ascertained, for it has not yet been described, it did not belong to the previously-described remains. It is impossible to build any theory on this last tooth, since it might have belonged to a man of comparatively recent period, and have come to lie where it did in any one of several ways, *e.g.*, by falling down a deep crack in the earth. The remains themselves have been assigned to a single individual named *Pithecanthropus erectus*, but apart from the initial difficulty alluded to above, the greatest difference of opinion exists as to the character of the skull. Dr. Munro† gives a list of seven authorities who look upon it as human, six who consider it to be simian, and seven who believe it to be a transitional form. Further, he quotes the following amusing paragraph, which exemplifies the

* See also p. 150.

† *Palæolithic Man*, p. 190. The points here mentioned are additional to those given on pp. 151-3 of this work.

discordance of opinion on the subject, from a paper by the veteran archæologist, G. de Mortillet, whose name has already been mentioned in these pages : " Les avis ont été on ne peut plus partagés. Ils se sont tout d'abord parqués par nationalités. Les Anglais, bien que compatriotes de Darwin, ont fait des grands efforts pour démontrer qu'il ne s'agit que d'un homme, un homme trés inférieur, mais déja un veritable homme. Les Allemands, au contraire, se sont froidement ingéniés a prouver qu'il ne s'agit que d'un singe. Les Françaises ont purement et simplement adopté les déterminations du jeune savant. C'etait chose facile pour des compatriotes de Lamarck."

Apart from, or rather in addition to, these unsolved difficulties, it is not certain whether the geological epoch of the stratum in which the remains were found belongs to pliocene or pleistocene times, the latter opinion being now, I understand, more in favour than it was. It is obvious that however much discussion may rage around these bones, and quite legitimately rage, no stable theory can be reared upon the very unstable footing which is now presented, until some fairly certain conclusion is arrived at with regard to these controverted points. At the same time it must be remembered that in connection with the Neanderthal skull, even in 1901 Schwalbe* was able to tabulate four distinct views, with several sub-divisions in each, as to its character. Yet more recent discoveries have cleared up the difficulties,

* See p. 148.

and there is but little if any difference of opinion on this skull at the present moment.

One other point may be dealt with here. The cubic capacity of the skull is, generally speaking, a measure of the skull-contents, *i.e.*, the brain. It is generally considered that size of brain and amount of intellect have some ratio, though perhaps it would be more correct to speak of the extent of the grey cortex. At any rate it is generally conceded that an estimate of the intellectual position, or at least possibilities of a race, may be gathered from their cranial capacity. There are remarkable exceptions to this rule, Paulina Musters, for example. Even a more striking case is that of Gambetta, who will certainly not be accused by anyone of having been deficient in what is commonly called "brains" Yet his brain weighed only two and a half pounds, the average British brain being about three pounds. As a matter of fact, Gambetta's brain-weight fell considerably below that of the average of savage races. However, with this word of caution, it may be said that skull-capacity is the best, and indeed almost the only, measure which we have of intellectual possibilities in otherwise unknown races. This statement is made with all caution, and with all reservations, as to relative size of different parts of the skull, and consequently brain. The rule in question can only be regarded as a rough approximation, but it is the best that we have.

In the case of the Trinil skull, which is very imperfect, it is exceedingly difficult to make ar accurate estimate of what was its original capacity.

Dubois put it down at eight hundred and fifty-five cubic centimeters, but Keith thinks that is an underestimate, though he does not commit himself to any figure, no doubt wisely, for the estimation can be little more than a guess. It may just be mentioned that the Australian savages' skull-capacity runs down to about one thousand cubic centimeters.

The Piltdown Skull. This skull, in an imperfect condition and with half a lower jaw, was found by Mr. Dawson in a flint-bearing gravel overlying the Wealden (Hastings beds) at Piltdown in Sussex. and was described by him and by Dr. Smith Woodward.* It is at this moment an object of active controversy. Its describers think that skull and mandible " cannot safely be described as being of earlier date than the first half of the Pleistocene epoch." There is some doubt as to whether the fragment of skull and the mandible belong to the same individual. Chellean implements were found with the remains, and are claimed as being of the same date. As to the skull itself, or rather the fragments which remain, it is stated that the cubic capacity is above that of the modern Australian savage, but in connection with the Piltdown example, it is a curious fact that the reconstruction of the skull carried out by the authors of the papers does not at all please Professor Keith, another high authority. This authority says that Dr. Smith Woodward's reconstruction is one of a man who " could neither breathe nor eat, which was

* *Quarterly Journal*, Geological Society, London, March, 1913, and April, 1914.

an absolutely impossible condition. The mistake had been made similar to that in 1887, of putting a chimpanzee face on a human skull" And he also states that the cubic capacity was one thousand five hundred and sixty cubic centimeters, in other words, that it was a very large skull, whilst the authors of the paper say that it was "at least one thousand and seventy cubic centimeters," a very great discrepancy in description. Whichever may be right, it is clear that it is a human skull with which we have to do, and (1915) it now seems to be accepted as a skull in no essential way differing from modern examples. Far more remarkable features attach to the mandible, and that may be considered in connection with another specimen which it somewhat resembles, namely,

The Heidelburg Jaw. Found near the place after which it was named, and first described in 1908, this jaw and that found at Piltdown resemble each other, in being more like a simian jaw than any others associated with human beings; yet both of them are believed to be human in their character, chiefly because the teeth are obviously human. What is still more remarkable is that they are actually more like the teeth of the higher races of man to-day, and less like the teeth of apes than are the teeth of some of the savage human races of to-day. This has been stated by Sollas of the Heidelburg jaw, and the teeth of the Piltdown specimen are claimed as definitely human. In some accounts of this specimen it is stated that the anatomical conditions point to the inability of the former possessor of the jaw to speak, but this

has been shown to be a mistake. There is no doubt that the Piltdown jaw and the skull, if the two belong to each other (which is thought by some to be unlikely), form in many ways a great puzzle, and one which is by no means cleared up. Perhaps further discoveries may throw light on the matter, as was the case with the Neanderthal skull.

Whilst dealing with this specimen, allusion may be made to the matter of reconstructions, since Professor Keith in his book has actually attempted to reconstruct the entire Heidelburg skull from the mandible which alone has come to hand. Of course it is most natural that anatomists should undertake tasks of this kind, and they have various rules and facts to aid them in carrying out their operations. But after all a great deal must be left to surmise, and the results obtained differ within too wide limits, far too wide limits, even to fall within the province of the law of error; in other words, they cannot be depended upon. A friend once suggested to me that a small committee of anatomists might very carefully measure a modern skull and take a cast of the same. Having done this, they might then cut away the parts missing in the Piltdown skull, and hand the fragments to some of the reconstructors of ancient skulls to work upon. The results compared with the cast and measurements would afford very interesting comments on the value of reconstructions, and would act as a splendid example of a control experiment. But perhaps it never will be carried out. Respecting the Piltdown skull, at this moment probably the most interesting remnant of humanity

under discussion amongst scientists, it may be said, first, that it seems likely that it dates back to a period of extreme antiquity, though it may be impossible to translate this statement into any actual number of years in such a manner as to stand criticism. Secondly, it may also be said that the skull is that of a man, and, even more, that the skull is of a character not inferior to that of races now in existence on this earth. Further, the implements found with it, if indeed they be the implements of the race to which the former owner of the skull belonged, are definite human instruments of a kind quite familiar to students of prehistoric archæology.

Later Examples. The specimens which have been so far engaging our attention are of a more or less isolated character, at least in our present state of knowledge, and present, as has been indicated, many features difficult to explain, and perhaps incapable of complete explanation. Such was the case, up to what we may fairly call the other day, with regard to some of the other specimens of the remains of early man on which recent discoveries have thrown a flood of light. Notably, as already mentioned, was this the case with regard to the Neanderthal skull. This fragment has been shown to possess characters so closely resembling those of other crania of an early date, that anthropologists now speak of a Neanderthal race from the name of the spot where the first and most celebrated example was discovered. This, again, is only one of several races at present believed to have been in existence at a very early period, such

as the Cro-Magnon Race, the Mousterian Race, and, at somewhat later but still very remote eras, the Aurignacians and the Magdalenians. These papers were not intended to contain an account of the discoveries of anthropologists, save in so far as they touch on a few fundamental problems, which were indicated in the first pages. Hence no attempt will be made to deal with these various races, other than to sum up in a few words the principal matters of interest which arise in connection with them. In the first place, it may be said that at whatever date or period they may have lived, they were unquestionably men, and that they possessed skulls of a cranial capacity not inferior, and in some cases—even very ancient cases—superior to that of races reckoned to-day as amongst the highest in existence. They had certain racial characteristics, but so have the peoples of the world to-day, and just as the Tartar differs from Negro, and Negro from European, so there were different races of mankind even at these very early periods. Such is the conclusion at which science has arrived at the present date.

In the second place the remains of their handicraft which have come down to us, prove conclusively that they had not only the skulls but the hands of man, and very skilful hands too, as will be admitted by any person who has examined the highly-finished implements which have come down to us. Let any person who examines some of these implements of flint, set himself down to the task of endeavouring to imitate them with the same materials and the same tools—pieces of stone—

as were used by primitive man, and he will find his respect for the craftsman of bygone days enormously raised as the result of his own failures to accomplish anything like what his far-off ancestor was able to achieve. The same fact is impressed upon us by the remarkable discoveries which have been made in connection with the artistic capacities of some of the earliest races of mankind.

Of the art of the earliest peoples known to us, we have at present no knowledge. Perhaps they lived in too strenuous times, and had too severe a struggle to maintain their existence, to devote any time to what is after all not a necessity of life, namely, art. For, as will be readily understood, the pursuit of art connotes a certain relief from extreme strain. When a man takes the trouble to decorate his weapons, it means that he has moments when he can feel sure that he will not be called upon to use them for their primary purpose. At any rate, it is only towards the later Palæolithic Period that we begin to find undoubted and extensive evidence of a love for and a great skill in pictorial art. This again is not a matter over which it is possible here to linger, but those who take the trouble to examine the numerous reproductions of this early art which are now available, will be struck by its excellence, its spirit, and its admirable reproduction of the great beasts and creatures amongst which man lived, with which he had to war, and of which he made his food.

Finally it may be said, that as far as we go back amongst the races to which allusion is now being made, we discover undoubted evidence of a belief

in the future life of man, and thus—so far as this is evidence—of the possession of religious beliefs. And considering how little we know about these far-off people, this is a great deal. Supposing that everything in these countries could be swept away except our graveyards, and that some after-coming race, ignorant of the customs of its forbears, were to examine them, the savants of that race would hardly be able to say much more than that the people whose cemeteries they had been examining believed in a future life. Of these far-off inhabitants of the world whose condition we have been inquiring into, we have nothing but the cemeteries or interments in caves to guide us, and yet of them we are able to make an exactly similar statement.

Thus we may sum up by saying with regard to all these peoples, that, judged by every standard, they were men like unto ourselves, though in many ways perhaps—in some cases quite certainly—of a more rugged cast, and though unprovided with the resources of civilization now at our disposal.

The Question of Date. A few words may perhaps be devoted to this important subject, which will be dealt with more fully in the next succeeding article. It has already been seen that enormous, quite possibly insuperable, difficulties surround the task of endeavouring to translate geological periods into actual numbers of years. How impossible this is, may be gathered from the fact that every book which has attempted the task discloses a different scheme of chronology.

Let it be clearly understood that as to relative

epochs, there is comparatively little difference of opinion. Some may doubt whether, *e.g.*, the Red Crag is a Pliocene or a Pleistocene formation, and others may hold differing opinions as to the number of inter-glacial periods, but on the main question of the succession of periods there is fairly general consent. It is when geologists—and still worse anthropologists—try to set these periods down in terms of years, that we enter the domain of chaos. But with all this it may be said quite definitely, that the point of appearance of man upon this earth must be put back to a very much greater distance of time than was dreamed of by writers up to a comparatively recent date. In this there is nothing whatsoever to disturb the mind of any Catholic. The *Catholic Encyclopædia* deals with this matter in a very carefully written article, which is no doubt accessible to most of the readers of these pages, and from which, therefore, only the following quotation may be made for the sake of those who may not have the volume at hand. The writer says (*sub voce* Chronology) *Creation of Man*:

"The question which this subject suggests is: Can we confine the time that man has existed on earth within the limits usually assigned, *i.e.*, within about four thousand years of the birth of Christ? The Church does not interfere with the freedom of scientists to examine into this subject, and form the best judgment they can with the aid of science. She evidently does not attach decisive influence to the chronology of the Vulgate, the official version

of the Western Church, since in the Martyrology for Christmas Day the creation of Adam is put down in the year 5199 B.C., which is the reading of the Septuagint. It is, however, certain that we cannot confine the years of man's sojourn on earth to that usually set down. But on the other hand, we are by no means driven to accept the extravagant conclusions of some scientists."

With these words we may fully concur.

VIII: SOME RECENT WORKS ON THE ANTIQUITY OF MAN*

AMONGST many other cogent reasons, the fact that the question of the antiquity of man upon this planet is inextricably mixed up with the problems of the Glacial Period makes the consideration of that era in the world's history one of the most interesting amongst the subjects at present under debate in scientific publications. It is enshrouded in darkness, yet there are rifts in the cloud through which the sky can clearly be seen, and the chief object of this review is to indicate what things are and what are not tolerably clearly proved or at least provisionally accepted by those with most right to speak on the subject.

It must be admitted that it is not their voice which is usually heard by the general public. The discovery of ancient human remains—and of late years we have had several such of the first interest—forms far too tempting a subject for the journalist to neglect, and, as he naturally desires to make his account as picturesque as possible,

* HOERNES.—*Der Diluviale Mensch in Europa.* 1903.
SOLLAS.—*Ancient Hunters.* Macmillan, 1911.
KEITH.—*Ancient Types of Man.* Harpers, 1911.
DUCKWORTH.—*Prehistoric Man.* Cambridge University Press, 1912.
WRIGHT, G. F.—*Origin and Antiquity of Man.* Murray, 1912.
GEIKIE, JAS.—*The Antiquity of Man in Europe.* Oliver & Boyd, 1914.
WRIGHT, W. B.—*The Quaternary Ice Age.* Macmillan, 1914.

he will not hesitate to accept the wildest of statements and to put them forward as if they were indisputable truths. Meantime the real authorities on the subject are admittedly groping their way, full of uncertainty, and amidst the most difficult and conflicting facts and theories.

The books set down at the head of this review represent the work of authorities of the first class : let us see how far they agree and to what extent we may consider the questions with which they are concerned as in any way settled. We may start with the fact, undisputed to-day, that there was an Ice Age or Glacial Period, which occupied that era of Geological time called Pleistocene. As to what caused that condition of affairs men of science are not in any kind of agreement, and as the questions involved in a discussion on that point involve a knowledge of physics and of astronomy which the present writer does not possess, no attempt will be made to deal with this matter. Nor is it in any way necessary for the purposes of this paper once more to recapitulate the evidence upon which our belief in the existence of an Ice Age depends, nor to describe the familiar objects which exist to-day to bear witness to its work.

During this period a vast area of Europe and North America, not to speak of other parts of the earth which do not come into consideration in this paper, were covered by a sheet of ice, as to the extent and thickness of which there is little difference of opinion amongst those who have given attention to the question. Speaking

generally, and neglecting for the moment the oscillations in temperature which will subsequently be dealt with, the lower edge of the ice may, in the words of Sollas, be described in Europe as stretching from Kerry to Wexford, and through the Bristol Channel to London, where it crosses the sea and continues its course through Antwerp, past Magdeburg, Cracow, Kiev, runs south of Moscow to Kazan, and then terminates at the southern end of the Ural mountains. All that lies to the north of the line in question, that is, the greater part of the British Isles, Northern Germany, Scandinavia, and almost the whole of European Russia, was buried out of sight beneath a mantle of ice formed by the confluence of many colossal glaciers (p. 11). The area thus covered was not less than two million square miles and the average depth of the ice seems to have been fully one mile (Wright, G. F., p. 162). As to North America, the great terminal moraine which marks the southern boundary of the ice can be traced with occasional interruptions from Nantucket, through Long Island, past New York, towards the western extremity of Lake Erie; then along a sinuous course in the same direction as the Ohio, down to its confluence with the Mississippi; then it follows the Missouri as far as Kansas City, and beyond runs approximately parallel to that river, but south of it, through Nebraska, Dakota and Montana, and Washington, where it meets the coast north of Columbia river. Within this boundary nearly half of North America was buried beneath a

thick sheet of ice, flowing more or less radiately outwards from a central region situated in and about the region of Hudson Bay (Sollas, p. 11). As to the extent of the American sheet of ice, it is stated that the area covered was fully double that in Europe, amounting to about four million square miles; while its depth is variously estimated to have been from one to three miles. The ice certainly was more than one mile deep over New England, for marks of the movement are found on the summit of Mount Washington, which is more than six thousand feet high (Wright G. F., p. 162).

The duration of this period will be considered further on; meantime it may be stated that it is tolerably generally* agreed that the Ice Age was not one of uniform and continuous cold. On the contrary, there were warmer intervals during which the enormous glaciers receded and portions of the earth till then locked up under arctic conditions became once more habitable by man and other mammals. This is not to say that all the ice disappeared, but that conditions more or less approximated to those which now exist. Indeed, since we know nothing really about the cause of the Ice Age, and as we are obviously now existing in a genial period, there is nothing to prove that it may not be merely an "interval," and that a fifth Glacial Period may not come upon this earth and wipe all the civilisation

* But not unanimously, for whilst some admit but one interglacial epoch, others doubt even that. See Lamplugh—Address, Sect. C, Brit. Assoc., 1906.

of Northern Europe and America off the face of the globe.

It will be necessary to say something about the glacial periods and the genial intervals, but since this article is chiefly concerned with the question of man, before this can be done it will be desirable, first of all, to sketch as briefly as possible the various races or successions of mankind during the Prehistoric Period. We shall then be in a position to understand the views put forward as to their relationship to the different epochs of the Glacial Period. Two matters, namely, the Trinil remains, the so-called Pithecanthropus Antiquus; and the question of eoliths having been already discussed at sufficient length need not further be dealt with here.

Passing from the possible eolithic period, we find ourselves confronted with two sub-divisions of the prehistoric age, Mesvinian and Strepyan, accepted as definite epochs by Sollas, but included as early *Chellean*, with the first generally accepted epoch, by perhaps the majority of authorities. Up to comparatively recent times it might have been said that Chellean man was known only by his implements, of which by far the most important and characteristic was the *coup-de-poing*, or "boucher," to use Sollas' term, a kind of hand-axe of stone not intended for insertion in or attachment to a handle. But there are two exceedingly important fragments of human remains which it seems may be at least tentatively associated with this period. The first of these is the so-called Heidelberg mandible, found under

eighty feet of sand and gravel at Mauer, near Heidelberg; and the second is the Piltdown skull, at the moment so fruitful a matter of discussion. As to the mandibles, in both cases this may be said that in general structure and appearance they are more simian than any human jaws which have yet been examined. Without entering into anatomical considerations, which would detain us too long, this fact may be admitted. Yet in neither case is the mandible actually simian, a fact admitted by Keith :—" The jaws of these early human beings were primitive enough, but certainly not simian. At even this early stage the simian condition was long past " (p. 88).* What is really remarkable is that in both cases apparently, but certainly in that of the Heidelberg mandible, the teeth are much *less* simian than those of certain races of mankind in existence at the present day. What explanation there may be of this apparent anomaly has not so far been discovered, yet (as in the case of the Neanderthal skull, once so great a puzzle, and an apparent anomaly)† we may hope for a clearing up of our

* It will of course be noted that he assumes the evolution of man from a simian form, the positive evidence for which so far as skeletal discoveries go, may be said to be almost entirely lacking. This matter cannot be discussed in the present paper.

† It is not necessary here to discuss the history of scientific opinion in connection with the Neanderthal skull. Some account thereof to the date of the article is given by the present writer in a previous article. Suffice it to say that after a lengthy discussion and great difference of opinion, including views of the most opposite character, more careful anatomical study seems to have proved that this skull, once thought, if human at all, to be that of a human being inferior to any living individual, is actually of a character not greatly or seriously differing from that of modern races. See p. 147 seq.

difficulties by fresh discoveries which may be made any day. At any rate these, the earliest certain remains of man known to us, reveal to us man not differing in any important particular from man as we now know him. If Professor Keith is correct in his most recent statement as to the Piltdown man and his cranial capacity was really 1,500 cc., his skull is well up to the average of the civilised races of to-day.

The Chellean and *Acheulean* periods, during the latter of which somewhat lighter and smaller implements were made than during the former, make up the Older Palaeolithic Age. It is succeeded by the Middle Palaeolithic, which is usually regarded as being conterminous with the *Mousterian* zone of civilisation.* The *coup-de-poing* has now disappeared, and broad and thick flakes worked on one side, with side-scrapers and points for use in the hand, are the characteristic implements.† Now, of Mousterian man we know a great deal, and we know of him, it may be said, in the mass or as a race, and not by mere isolated fragments, as was the case with the earlier examples. For of Mousterian man we have now quite abundant remains, including that once much-debated specimen, the Neanderthal

* It is perhaps hardly necessary to say that the terminology of these zones of civilisation is derived from the places, mostly French, where the chief or most typical examples of the skeletal remains, or in some instances, such as Chelles, the characteristic implements have been found.

† The reader will find an excellent and brief table of the zones in question in Wright, W. B., p. 254, and as this is the first time that this book has been quoted it may be permissible to commend it to readers as a most admirable, complete, and up-to-date account of the subject with which it deals.

skull. Briefly, it may be said of him that as far as cranial capacity goes he was on an equality with and even at times in advance of man of the present day.* Further, he had the hands of a man, as his wonderful craftmanship in flint undoubtedly proves. And, finally, he believed in a future life, as is shown by the offerings which he buried with his dead. We can thus reconstruct the Mousterian physically and to some extent psychologically, when he appears before us a man and in every sense of the word. Passing next to the Younger Palæolithic time, we find four generally recognized zones of culture (or races, if that term pleases better), which in order from below are :

Aurignacian, which interests us because of the sudden appearance of a most remarkable outburst of art, both decorative, as in the caves, and applied, in connection with implements.†

Solutrian, sometimes regarded as a part of the last, during which time the working of stone—

* Cranial capacity though not an infallible indication is the best we possess as to size of brain, and consequently intellectual capacity. Yet there are tremendous pitfalls here. The average European capacity is 1,550 cc. Mousterian man averages 1,600. Bismarck's capacity was 1,965, that of Leibnitz was 1,422, Gambetta's brain weighed only 2½ lbs., the average weight of a British brain being 3 lbs. Gambetta's brain was inferior in size to that of the ordinary savage, and one wonders what would have been said about his skull had it turned up as a prehistoric object! It is clear that only in a very general way can cranial capacity be relied upon as evidence in relation to intellectual position. As far as it goes it is all on the side of the Mousterians.

† Apart from the account of this art which is to be found in Sollas, readers may be referred to a number of papers in L'Anthropologie, and, for a fairly full description with numerous reproductions, coloured and otherwise, to Spearing, *The Childhood of Art*, Kegan Paul, 1912.

so far as the Palæolithic Period goes—attained its maximum of excellence.

Magdalenian, a period of great development in connection with work in bone and in horn, and also in the art of drawing.

Tourassian, during which stone work declines, as does that in bone; whilst implements carved in flat stag's horn, such as harpoons, are very characteristic.

Then follows the *Neolithic* period, as to which this may be said, that whilst it is obvious that there must have been a continuity of the human race somewhere—and there are substantial pieces of evidence which tend to prove that that continuity can be established in certain parts of France*—there was, as will further be shown, a complete hiatus in our part of the world. As far as Ireland is concerned, there is no certain evidence of occupation by Transition people, still less by any representatives of the Palæolithic Age. Such is not the case in England, for W. B. Wright gives the following useful list of caves in England where Palæolithic remains have been discovered (p. 281):

Chelleo-Mousterian.—Alluvium of the Thames, Ouse, and Avon; Brixham Caves and Wookey Hole, in Somerset; Kent's Hole, near Torquay, in Devonshire; Robin Hood's Cave, near Creswell, in Derbyshire.

* The discoveries of Piette at Mas d'Azil which are here referred to will be found in full detail in the volumes of L'Anthropologie. See also Geikie, p. 294, *seq.*, Wright, W. B., p. 284, *seq.*

Solutrian.—Robin Hood's Cave and Church Hole, near Creswell; Kent's Hole.

Magdalenian.—Robin Hood's Cave, Church Hole, and Kent's Hole.

To which may be added:

Aurignacian.—Paviland Cave, in Gower, recently described by Sollas.

A further point of much interest and very fully worked out in Sollas' book is the attempt to show a racial connection between some of these Palæolithic races and existing peoples of to-day. "If the views we have expressed in this and preceding chapters are well founded," he says (p. 382), "it would appear that the succeeding races, which represent the vanished Palæolithic hunters, have succeeded one another over Europe in the order of their intelligence: each has yielded in turn to a more highly developed and more highly gifted form of man. From what is now the focus of civilisation they have been one by one expelled and driven to the uttermost parts of the earth: the Mousterians survive in the remotely related Australians at the Antipodes; the Solutrians* are represented by the Bushmen of the southern extremity of Africa; the Magdalenians by the Eskimo on the frozen margin of the North American continent, and as well, perhaps, by the Red Indians."

However this may be, it seems that all these zones or races are associated with the Glacial

* With whom must be included the Aurignacians who are, perhaps, more nearly akin to the Bushmen than the Solutrians.

Period, for it is still true, as Sollas states (p. 69): "On the important question of man's first arrival on this planet we may for the present possess our minds in peace, not a trace of unquestionable evidence of his existence having been found in strata admittedly older than the Pleistocene." Hence it becomes of interest to make the attempt to place these different races in their proper relationship to the various epochs which have been assigned to the Ice Age. This can only be done by the consideration of a number of different arguments which cannot be more than indicated here, but amongst which the most important is the character of the animal and to a less extent vegetable remains found in connection with those of human character, whether skeletal or implements, such giving important indications as to the kind of climate which existed at the period to which the implements and other relics belong.

Though there are differences of opinion on this point, the general view is that there were, during the Ice Age, four periods of intense glaciation, with three milder intervals, and succeeded by a fourth mild period, namely, the present and recent epoch. Various names have been given to these periods, and the following table will exhibit the classification of Geikie (pp. 248 *seq.*), associated with which is the Alpine classification of Penck and Brückner (P.). The relation of human races, according to Geikie (G.) and Hoernes (H., see p. 8 for his classification), to the various periods at least illustrates the differences of opinion which

exist and the extreme uncertainty in which the whole question is involved.

First Glacial Period.—Scanian (G.); Günz (P.)

Commences at the beginning of the Pleistocene period, and was an epoch of intense cold, all records of which in Britain have been destroyed or buried under later glacial formations.

First Inter-Glacial Period.—Norfolkian (G.); Günz-Mindel (P.).

Britain was joined to the Continent, and the Thames was a tributary of the Rhine. The hippopotamus, elephants (*E. meridionalis* and *E. antiquus*), bear, bison, sabre-toothed tiger were amongst the fauna. Geikie (p. 250) thinks that the sand-beds of Mauer, in which the Heidelberg mandible was discovered may belong to this period. "The geological and palæontological evidence, although not quite decisive, seems to favour the reference of this ancient human type to the First Interglacial or Norfolkian stage."

Hoernes associates both Chellean and Mousterian races with this interval, which, it may be added, corresponds to the Cave-Bear Period of E. Lartet.*

Second Glacial Period.—Saxonian (G.); Mindel (P.).

Though not so severe, the diminution in temperature was considerable, as much as 20°, "a

* In order to prevent this account from becoming inordinately long and involved, I omit the opinions of other authorities as to the place of the different races in connection with the Glacial periods. Suffice it to say that there is great difference of opinion, and some account thereof will be found in Sollas, pp. 398, *seq.*

change as great as we now experience in passing from the south of England to the North Cape" (G., p. 252). In England the ice-sheet extended as far south as the Valley of the Thames.

Second Inter-Glacial Period.—Tyrolian (G.); Mindel-Riss (P.).

During the recession of the ice at the end of the Saxonian period tundra conditions obtained, and the reindeer, mammoth, musk-ox, and woolly rhinoceros were prominent amongst the fauna. In the inter-glacial period which followed the climate was milder than at present, and the snow-fields and glaciers of the Alps were much less extensive than they are now. There were land-bridges connecting Britain with the Continent and Europe with Africa; "hippotamus, elephants, rhinoceroses, cervine and bovine animals, and many carnivores ranged over the major portions of Europe" (G., p. 254). The Victoria Cave at Settle is one of the many and perhaps the most prominent instance of a place containing relics of this period, for there "remains of the southern and temperate mammals occur, as hippotamus, rhinoceros (not the woolly rhinoceros), straight-tusked elephant, bison, red deer, hyaena, etc." (*ib.*, *ib.*) Geikie associates this epoch with the Chellean zone of civilisation, whose "rudely fashioned stone implements seldom occur in caves, but are often met with in the older Pleistocene river-drifts. From this it has been inferred that Chellean man probably lived in the open, for the climate was clement and equable,

the seasons not being so strongly contrasted as in our days. The margins of the rivers were apparently favourite haunts of the race, the coarse gravels affording an inexhaustible supply of the stones required for implements" (*ib.*, *ib*).

On the other hand Hoernes, who had already disposed of the Chelleans, and even of the Mousterians in the earlier period, places the Solutrian zone of civilisation here. It corresponds with the Mammoth Period of E. Lartet. It seems to have been the longest and the warmest of all the intervals, and as the climate gradually grew colder, the hippopotamus, straight-tusked and southern elephant migrated southwards, the last-named never to return. It was at this time, as Geikie thinks, that the Acheulean zone of civilisation came into existence and ran its course.

THIRD GLACIAL PERIOD.—Polonian (G.); Riss (P.).

During the gradual transition from the Tyrolian to the Polonian periods, and before the climax of the latter, Geikie thinks that "men of the Mousterian stage of culture had come to occupy the caves of north-west, central, and southern Europe. In England, Belgium, France, and Germany he was eventually contemporaneous, not only with mammoth and woolly rhinoceros, but with reindeer, glutton, arctic fox, and other members of the tundra fauna." This sheet of ice did not extend so far south in England as that of the Saxonian period, which, it will be remembered, reached the valley of the Thames.

The Polonian sheet came only as far south as the midlands.

Third Inter-Glacial Period.—Dürntenian (G.); Riss-Würm (P.).

During this genial epoch the Alpine glaciers dwindled considerably, and in the cave of Wildkirchli, which is 4,800 feet in elevation, on the Ebenalp, have been found, along with implements of Mousterian character, a fauna amongst which are cave-bear, cave-lion, wolf, stag, ibex, all which species have been identified in the Mentone caves, where they are also associated with Mousterian implements, as well as with the remains of the straight-tusked elephant, the broad-nosed rhinoceros, and the hippopotamus (G., 263). Geikie associates the Mousterian civilization with this period. On the other hand, Hoernes places Magdalenian man in it, a race certainly much later than the Mousterian and now invariably included amongst the Younger Palæolithic races, and not the Middle, as is the case with the Mousterians. Keith assigns to this period the Galley Hill skeleton, a specimen as to which there has been great controversy, discovered in the Thames Valley some years ago, and remarks concerning it: "The first impression on examining the remains of this earliest known inhabitant of England* is one of surprise, almost of disappointment; in all his features,

* At the date when the book was published this statement was correct, but it is some index of the rapidity with which discoveries have been made that it should no longer be accurate, since at least the Piltdown skull seems to be of much greater antiquity.

with a few exceptions, he is so modern in build that we might meet him on the streets of London to-day and pass him by unnoted" (Keith, p. 32).

This epoch corresponds with the Reindeer Period of E. Lartet, and it is divided, by Hoernes, into two sub-periods. In the first of these, the Reindeer Period, he places the Magdalenians, as already mentioned. But in the second, or Red-deer Period, he actually includes the Tourassians or Asylians, that is to say, he has now got far ahead of Geikie and, it may be added, most other authorities on the subject, having reached the zone of Transition.

Fourth Glacial Period.—Mecklenburgian (G.); Würm (P.).

"The younger archæological stages—the Aurignacian, the Solutrian, and the Magdalenian—are closely related to this epoch, the mammalian fauna indicating for the two first-named stages a somewhat cold climate (as in the cave at Mentone and elsewhere), and for the Magdalenian even colder conditions. Probably the two first-named stages should be assigned to the dawn of the Würmian—to the period of the transition from the preceding inter-glacial epoch, while the Magdalenian belongs essentially to the succeeding glacial epoch" (G., 267). Magdalenian man or his implements are found all over the Continent of Europe, in England, as well as in Belgium, Germany, Switzerland, Austria, Russian Poland, etc. But it is not necessary to believe, nor is it likely, that he inhabited these

lands contemporaneously. On the contrary, it seems likely that as the cold became more severe in the north he migrated southwards to middle Europe, where tundra conditions obtained. With the end of this period comes the disappearance of the Magdalenians, and at this point we enter a region of great uncertainty..

We know that the Neolithic or Polished Stone Age came after the Magdalenian : is there any connection between them, or is the hiatus complete, and was the Neolithic civilisation the property of a totally different race ? Of course no one doubts that somewhere or another the hiatus was bridged over, for no one argues that when Palæolithic man came to an end a perfectly new race unconnected with him came into being. But, so far as we can judge, in the northern part of Europe, at any rate, Palæolithic man did disappear off the face of the earth, which remained uninhabited by man at least for a prolonged period. " It is beyond question," writes W. B. Wright (p. 78), " that in post-glacial times neither Ireland, Wales, nor the northern half of Great Britain were occupied by man until the long subsequent Neolithic invasion. Even the south of England affords evidence of this general exodus, for here there is a complete break both in stratigraphical relations and style of workmanship between the implements of the two periods. Between the Palæolithic and Neolithic culture of Great Britain there is a great gulf fixed, and no amount of research has succeeded in finding any trace of a transition between the

two." Perhaps all prehistoric archæologists would not be quite as emphatic as Mr. Wright, but it must be admitted that up to the present, in spite of various and even vigorous efforts, no one has succeeded in convincing the scientific world that the opinion embodied in the quotation just given is inaccurate. If, then, there is a complete hiatus so far as the districts above named are concerned, is there any place where the transition between the two types of civilisation can be observed? As already stated, there seems very good reason to believe that Piette has discovered such a place in the grotto of Mas d'Azil, from the remains in which a certain zone of civilisation is now known as *Asylian*.

Further transition zones are known as *Campignian*, *Tardenoisian*, *Tourassian* (the same as Asylian), and *Arisien*. It must be noted that this period of transition, according to Geikie, belongs to the fourth mild epoch, in which we are now living, but according to Hoernes to the fourth or Mecklenburgian Glacial Period. It is only one of several serious discrepancies between the two classifications.

Before leaving this part of the paper and turning to the last consideration, namely, that of the time occupied by the Ice Age, it may be well to quote the comparison between the Magdalenian or latest Palæolithic civilisation and that of the Neolithic Period, since it shows vividly how great the gap is which has to be bridged over by the transition stage wherever it may have existed.*

* The table is from Wright, W. B., p. 284.

Magdalenian.	*Neolithic.*
Poorly developed stone industry	Highly developed stone industry, art of grinding & polishing stone.
Remarkable skill in drawing and etching	Absolutely no pictorial art.
Hunting and fishing the only occupation.	Agriculture and the keeping of domestic animals.
A cold fauna with reindeer and mammoth.	The fauna of the present day.

It is inevitable that efforts should be made to express the periods with which we have just been dealing in actual terms of years as we are able to do, for example, in the case of the different families or dynasties which have occupied the throne of England. The extraordinary discrepancies which exist between the computations of different observers is in itself quite sufficient evidence of the hopeless character of such an effort; but in order that this point may be brought out more fully it will be as well to quote the figures assigned by different writers.

Before doing so, however, it is only fair to point out that geologists at any rate—and it is to them that we must look for real information on this point—are unanimous in asserting the precarious character of any figures which they may set down, and in stating that they cannot pretend to do more than indicate in a very rough manner the comparative lengths, or what they

think to be the comparative lengths of the different periods. "No geologist has overmuch confidence in such estimates," says Geikie (p. 300); and adds that "they serve to give some precision to our conception of geological time." It must be admitted that the precision is not very precise when the enormous discrepancies between different views is considered.

Let us begin by considering the post-glacial period, that in which we are now living. There was a time when the receding ice left parts of Europe habitable whilst others were not so—when was this? Some would place it at a quite recent period, for example, G. F. Wright, who maintains that "large areas in Europe and North America which are now principal centres of civilisation were buried under glacial ice thousands of feet thick, while the civilisation of Babylonia was in its heyday" (p. 195), a period which may be set down at about 3,000 to 4,000 years B.C. And he continues; "The glib manner in which many, not to say most, popular writers, as well as many observers of limited range, speak of the Glacial epoch as far distant in geological time is due to ignorance of facts which would seem to be so clear that he who runs might read them." Dr. Wright has been studying glacial phenomena for forty years, and has written much upon them, and such are his views; but they run directly contrary to those of many others who have also made glacial matters their life-study. I am not now thinking of the extravagant—admittedly extravagant—figures put forward, chiefly, it must

be admitted, by anthropologists, but to the figures of geologists, and of geologists who have specialised in glacial matters.*

Penck, whose researches have been carried on in the Alpine district, and who depends upon the evidence of denudation† for his estimates, may be regarded as the writer who amongst recognised glacialists demands the largest allowance of time, and Penck thinks that the post-glacial period in which we now exist cannot have lasted for less than 20,000 years. On the other hand, Sollas (pp. 393 *seq.*), after tracing the history of mankind backward for 7,000 years, comes to Asilian time, and states that "from this point —the beginning of the seventh millenium—we look backwards over the last glacial episode."

De Geers, whose observations on the rate of deposit of the laminated marine clays of Southern Sweden at present seem to hold out some hope of our having at last obtained a reliable geological clock, claims that he has shown that it is 9,000 years since the ground on which the University of Stockholm stands became free from ice (Wright, W. B., p. 343). In this connection it may be noted that a variety of "geological clocks" have been set up from time to time during the last sixty

* A remarkable example of the extraordinary differences of estimate in these matters may be found in the fact that Professor Sollas, a geologist, puts the Mousterian race of Chapelle-aux-Saintes at a distance of twenty-five thousand years, whilst Professor Keith, an anthropologist, will not be satisfied with less than three hundred and fifty thousand. It must be admitted that the discrepancy is remarkable. The fact is that no one has any real idea of the periods of glacial time in terms of years.

† Notoriously very dangerous and doubtful evidence, as all will admit.

years, but so far no one of them has proved itself capable of keeping anything like accurate time.* Further, it may be noted that de Geers has also quite recently brought forward evidence from a post-glacial lake-bed in Sweden from which he concludes that the ice did not leave that region until about five thousand years ago (Wright, G. F., p. 193).

So much for the post-glacial epoch, as to the duration of which it is obvious that there is considerable difference of opinion. But it is nothing to that which exists as to still earlier times. A few examples may be given to illustrate this fact before this paper is concluded.

We may return to Penck, because he is a great authority and the writer who draws the largest cheques upon the bank of time.

Penck demands for the genial intervals the following periods of years :

For the Riss-Würm, or Third Inter-glacial Period	60,000
For the Mindel-Riss, or Fourth Inter-glacial Period	240,000

As, allowing for 20,000 years for post-glacial time, we have now arrived at a point 320,000 distant from the present day ; and as we have made no allowance for the First Inter-glacial period, not to speak of the intervening glacial epochs, we have a very tidy bill of time to foot. But the other periods of time must be considered. " The data,"

* See p. 168 seq.

says Geikie (p. 301), "for determining the duration of the First, or Günz-Mindel, Inter-glacial epoch are not so ample—all the evidence, however, leads to the belief that while not so long as the second, it was much longer than the Third Inter-glacial epoch. We may provisionally assume its duration to have been about 100,000 years, and we thus obtain 400,000 years for the first three inter-glacial epochs, to which we may add 20,000 years to cover the interstadial stages of post-Wurmian times." Then we have still to allow for the glacial epochs themselves, and according to Professor Geikie we must allow not less than 200,000 years for these collectively, which gives us a "minimum of 620,000 years for the duration of Pleistocene times." And what part of this belongs to the history of man? "Quite recently Professor Penck has expressed the opinion that the Glacial period with all its climatic changes may have extended over half a million years, and as the Chellean stage dates back to at least the middle of the period this would give somewhere between 250,000 and 500,000 years for the antiquity of man in Europe. But if, as recent discoveries would seem to indicate, man was an occupant of our Continent during the First Inter-glacial epoch, if not in still earlier times, we may be compelled greatly to increase our estimate of his antiquity" (Geikie, pp. 302-3). Before turning to the more moderate geologists for their views we may ask the following question, which cannot fail to occur to anyone thinking over the facts which have been

detailed in this paper. On the showing of Penck we must go back, let us say, 200,000 years for Mousterian Man. Yet Mousterian Man, as we have seen, was a man in every sense of the word, for there is no reason to suppose that he was in any way less intelligent than we are to-day, though he had, of course, infinitely less advantages and many more difficulties to contend with. We have to go back, let us say, some 10,000 years, perhaps less, to arrive at Neolithic man, 10,000 years during which our present civilisation has been evolved, perhaps not 6,000 since metals came into use, or 3,000 or 4,000 since iron was discovered. What was man doing during that 190,000 years that his progress from the skilfully made implements of the Mousterian age should have been so lamentably slow? But we have other authorities whose demands of time are much more moderate, and we may take the most moderate of them as a contrast to the theories of Penck. G. F. Wright, as we have already seen, thinks that the end of the glacial epoch was at quite a recent date, and he adduces remarkable evidence from the rate of erosion of the gorge of Niagara. Like most waterfalls, Niagara is a post-glacial object, as is evident from the fact that "there is a buried preglacial channel leading from Lake Erie to Lake Ontario, some distance west of the present river" (Wright, G. F., p. 176). Now the gorge is at present about seven miles in length, and is composed of strata, as to which Wright remarks that "no geological conditions could be more uniform and calculated to

yield more definite results to careful study." We ought, therefore, to be able to secure valuable information from a consideration of the facts obtained over a number of years study of the conditions.

Lyell visited the falls in 1842, and made a guess —admittedly a guess, though it has often been quoted as if it were a carefully thought-out opinion—that the rate of erosion could not be more than one foot per annum, and probably would not be more than a third of this. This set geologists to work, and during the sixty-five years which had intervened between the date mentioned above and 1907 it turned out that the rate of erosion has been a little more than five feet per annum, that is, more than five times Lyell's maximum, and more than fifteen times his minimum. "So that, if the same forces had been at work continuously in the past that are operative at the present, Niagara River would have eroded the whole gorge in seven thousand years," says G. F. Wright (p. 178), and proceeds to show where the falls were at different points of history, terminating by the statement that "the beginning of the Falls at Queenston occurred only a short time before the building of the great pyramids and the expedition of Sargon from Babylonia to the shores of the Mediterranean about 3,800 B.C." From this and a vast amount of other evidence, for which the reader must be referred to his book, he concludes that the epoch—the entire glacial epoch—does not extend to more than 80,000 years (p. 200), and

as regards the history of man, he states that while his antiquity "cannot be less than ten thousand, it need not be more than fifteen thousand years. Eight thousand years of prehistoric time is ample to account for all known facts relating to his development" (p. 496). No doubt many will laugh at this moderate estimate, but it has strong supporters. The Abbé Breuil, for example, admittedly one of the foremost prehistoric archæologists of the day, is in accord with Driver, who thinks that the time of man's sojourn on earth cannot be less than 20,000 years. Prestwich even limited the entire glacial period—and thus, according to our present knowledge, still more limited the antiquity of man—to 25,000 years.

Whilst these extraordinary differences exist between the estimates of scientific men, those who (like the present writer) are not, from their own investigation, competent to express any opinion, may well hold their judgments in suspense and await the day when some kind of knowledge as to what caused the Glacial Period may begin to throw light upon the length of its duration.

That such knowledge may be arrived at we may look upon as more than possible; that it has been striven after long and oft is obvious from the books referred to in this article; that it has in no way yet been reached is not less obvious from the extraordinary discrepancies to which attention has been called.

IX: TOTEMISM AND EXOGAMY*

IT is now some forty years ago since McLennan, in his work on *Primitive Marriage*, first really attracted the attention of scholars to the two customs which form the title of the work under review. He was not the first to refer to these customs, since totemism as a term seems to have been originally introduced by J. Long, an Indian interpreter, in 1791, and the custom of exogamy, though that name was not then given to it, was outlined by Latham in 1859. But it may safely be said that it was not until McLennan's work had appeared that any real discussion over these matters arose.

Since then have appeared innumerable books and papers on these subjects, amongst which Baldwin Spencer and Gillen's accounts of the Central Australian Tribes, Frazer's article on "Totemism" in the penultimate edition of the *Encylcopædia Britannica*, subsequently published in a separate volume and republished as the first part of Vol. I of his present work, and Andrew Lang's *Secret of the Totem* (Longmans. 1905) may be specialy noted. Finally, these ripples on the sea of knowledge have been succeeded by the vast billow of *Totemism and Exogamy*, a four-volume

* *Totemism and Exogamy*: A Treatise on Certain Early Forms of Superstition and Society. By J. G. Frazer, D.C.L., etc. 4 vols, pp. xix—2,181, with maps. London: Macmillan and Co. 1910.

work containing over two thousand pages of reading matter and numerous maps, and embodying, as will be seen, a new theory of the origin of the first-named custom, new, that is, to the author's previous volume, but first given to the world in the columns of the *Fortnightly Review* in 1905 and reprinted in Vol. I. of the present work. The greater portion of three out of the four volumes is occupied by an ethnographical survey of the customs, as to the value of which there can be no second opinion. Many years must elapse before the time arrives when any ambitious writer will essay the task of compiling a collection of facts which shall render this book obsolete. And, as a collection of facts, it is indispensable to all persons engaged in the study of primitive races and their ideas. The fourth volume is devoted to summaries and conclusions. The former, expressing the essence of what has been learnt from the survey, contain those pages which will naturally be most referred to by the general reader and sum the evidence up in a manner of which it would be hypercritical to complain. It is when we come to the conclusions, the theory as distinguished from the facts, that we find cause to confirm the belief raised in our mind by the careful perusal of Professor Frazer's other works, that whilst he is, perhaps, the most painstaking collector of evidence in the world of scholars, he is not the most trustworthy theorist, but is apt to be led to what seem to us, with all respect be it said, utterly improbable conclusions by a too great reliance on isolated cases and far-fetched analogies. This in no way lessens the

debt of gratitude owing to him by scholars, who will greedily avail themselves of the quarry of facts which he has opened for them, but it does call for a note of caution to those who are inclined to place his theories as well as his facts in the same category of things unassailable. To this we shall refer in a later portion of this paper: at present we confine ourselves to giving some account of the two customs whose names appear at the head of this article.

"A totem is a class of material objects which a savage regards with superstitious respect, believing that there exists between him and every member of the class an intimate and altogether special relation" (1, 3). Apart from the material objects selected as totems, and these are innumerable, there are several varieties of totem to which reference must be made. Of these by far the most important is the Clan-totem, common to a class and passing by inheritance from generation to generation. The Crows are succeeded (within limits which will be indicated further on) by Crows and the Eagle-Hawks by Eagle-Hawks. Then there is the sex-totem belonging to all the men or all the women of the tribe but not to members of both sexes. Finally there is the personal totem, which belongs to a man or a woman and which is not handed down to his or her descendants.

We have already seen that the term totem is derived from a North American Indian source, a fact which is due to the accident that the custom was first noticed and described in that part of the

world. But it is by no means confined to the peoples which once inhabited the prairies and mountains of that vast area of land. Of late it has been carefully studied amongst the aborigines of Australia, where it appears to have occurred universally. It is met with amongst the western islanders of Torres Straits and the coast tribes of British New Guinea, very commonly amongst the Melanesians, to some extent amongst the Polynesians, and, perhaps, still more so among the Indonesians. "In India it is widespread, and may well have been at one time universal among the Dravidian races, who probably form the aboriginal population of Hindostan, and it appears to be shared by some of the Mongoloid tribes of Assam. But on the frontiers of British India the institution, or at all events the record of it, stops abruptly" (iv. 11). It is well known in Africa, as we have seen in North America and, it may be added, in the southern part of that continent as well. It will be expected that efforts have been made to show that it formerly existed in other races now presenting no traces of any such institution. Thus, Robertson-Smith endeavoured to trace it amongst the Semites, Reinach and others among the Celts and so on. It cannot be said that these efforts have so far met with any great success. The facts upon which these theories are based are not always unassailable, and, even when they may be taken as reasonably well established, a wholly different conclusion from that drawn by the totemists may with equal reason and probability be adopted. Perhaps one amusing instance of how these theories, based on

insufficient evidence, break down when exposed to expert criticism may here be given. The late Mr. Grant Allen, in his book on *Anglo-Saxon Britain*, which, with a real touch of humour, was published by the S.P.C.K., included Wormingford among the Place-names leading to "the almost irresistible inference that at some earlier period the Anglo-Saxons had been totemists" (p. 81, ed. 1891). In this hypothesis was, of course, involved the idea that Wormingford was the ford of the Wormings, and that the Wormings, after Kemble's theory, were the family or people of the Worm. Unfortunately for this view of things, Mr. Horace Round has had no difficulty in proving that Wormingford is, by a corruption of the name, the "Widemondfort" of Domesday Book and the "Withermundeford" of later charters and has nothing to do with Wormings or Worms.*

Respecting the races other than those included in the list of undoubtedly totemistic peoples, Professor Frazer concludes that, so far as he has studied the evidence adduced to support these conclusions, he has to confess that it leaves him doubtful or unconvinced (iv., 13), and with this view it is probable that most anthropologists will agree.

However, in the vast field of undoubtedly totemistic races there is abundant material for study and sufficient underlying similarity of custom to permit of scientific generalizations.

What, it may in the first place be asked, is the

* In a paper read before the Congress of Archæological Societies in Union with the Society of Antiquaries, 1900, and subsequently published in his *Commune of London and other studies*.

real relation of the man to his totem? On this point there seems to be no very great reason for doubt that in a number of cases a tribe believes itself to be actually descended from the totem it bears. Thus, for example, "the Cray-Fish clan of the Choctaws were originally cray-fish and lived underground, coming up occasionally through the mud to the surface. Once a party of Choctaws smoked them out, and, treating them kindly, taught them the Choctaw language, taught them to walk on two legs, made them cut off their toe-nails and pluck the hair from their bodies, after which they adopted them into the tribe, But the rest of their kindred, the cray-fish, are still living underground" (1, 5). From this view as to the kinship between a man and his totem would seem to arise the idea, which, by the way, is not in any way universal, that a man must not eat his totem-animal. The man, in fact, pays to the totem something like the same respect and consideration which he pays to his obviously human relatives, "hence, when his totem is an edible animal or plant, he commonly, but not always, abstains from killing and eating it, just as he commonly, but not always, abstains from killing and eating his friends and relations" (iv., 5). Even where the idea of descent is absent or obscured there is, or may be, an obvious recognition of a relationship between the man and his totem, and, on the man's side, a kind of feeling of camaraderie, coupled, at least at times, with the belief that he can, by the exercise of magic bring his influence to bear on the totem animal with which he is connected.

Thus he will perform ceremonies in order that the totem animal or plant may be miraculously multiplied, and thus an abundant supply of food be provided for those members of his tribe, though not of his clan of that tribe, who, owning another totem, may partake of his totem animal; and in the expectation that they, in their turn, will pay him the compliment of multiplying their totem animal for his benefit. It is quite obvious that, with the multiplicity of totem animals and plants, if the owner of a totem was called upon to defend it against the hunger of all other persons as well as to refrain from it himself, existence would become unbearable or impossible. So he does not object to his fellow feeding on the totem which he himself must avoid. Thus we get the *intichiuma* ceremonies of the Central Australians, where the native identifies himself, as far as possible, with his totem in order to attain the object in view. "If he desires to multiply grubs, he pretends to be a grub himself, emerging from the chrysalis state; if his wish is to ensure a plentiful supply of emus, he dresses himself up as an emu, and mimics the bird; for by thus converting himself into a grub, or an emu, he thinks he can move the other grubs or emus to comply with his wishes" (1, 119). But from this it must not be supposed that totemism is a religion or that totem is a god. Some have held the opposite view, and amongst them was Professor Frazer in earlier days, but he now makes it clear that he considers these views to be erroneous and that it is wholly improper to speak of true totemism as a religion (iv., 5).

Nor must we part from this point without alluding to the puzzling fact that, whilst some totemists may not and do not eat their totem, others not only eat it occasionally but believe that their ancestors did so frequently and in large quantities, and this with the idea in both cases that to eat the animal is in some sense to become the animal and, therefore, to gain greater power over its race-fellows. This discrepancy of idea affords a striking example of the difficulties which arise in the path of anyone endeavouring to correlate savage beliefs and draw wide generalizations from them.

We cannot spare the space to consider the question of the relation of decadent systems of totemism to forms of what may be called religion, but must content ourselves with calling attention to the fact that there is some evidence as to a causal nexus of this kind as, for example, in the region of Torres Straits (see 11, 18), where there are the shrines of two brothers, called Sigai and Maiau, who first appeared on the island as a hammer-headed shark and a crocodile, but seem later on to have assumed human attributes and are even now known to women and uninitiated, who are not allowed near the shrines, under their names and not as animals at all.

We must now turn to the subject of exogamy, the consideration of which is so inextricably mixed up with that of totemism. In its essence exogamy means "marriage without," *i.e.*, without the tribe or clan, and to make things simple we may say that it resolves itself into a less or more complicated Table of Forbidden Degrees.

In the first place let it be understood that by most ethnologists, and, at one time, though apparently not now, by Professor Frazer, it is held that descent in the maternal line is a more archaic system than that in the paternal; indeed it is not hard to understand why this should be when one considers many of the phases of savage life.

Professor Frazer, who firmly adheres to the idea of a primitive system of group-marriage which others refuse to admit, suggests (iv., 133) that the germ of exogamy "is the deliberate bisection of the whole community into two exogamous classes for the purpose of preventing the sexual unions of near kin." If this be correct we can imagine a tribe splitting into two divisions, one called Crow and the other Eagle-Hawk, and laying down the law that in future Crow shall not marry Crow nor Eagle-Hawk, Eagle-Hawk. It is obvious that such a system would be exogamous in its character, and it is notorious that such a system obtains in most, though not in all, of the totemistic peoples. There are, however, further complications in other cases. Let us, with Professor Frazer, deal with the matter by the use of letters, and let us call the two divisions of the tribe A's and B's. Then the plan mentioned above resolves itself into the law that an A must marry a B; this is a two-class system. But there is a four-class system under which A is again subdivided into a^1 and a^2 and B into b^1 and b^2 and under which also, though A must still marry B, he may not marry any B, but must marry if Aa^1 a Bb^1, if Aa^2 a Bb^2. There is even an eight-class scheme, where, of course, further prohibitions

exist and greater complications arise. Let us see what these regulations amount to in terms of ordinary relationship. "The effect of the two-class system is to bar the marriage of brothers and sisters, but not in all cases the marriage of parents with children, nor the marriage of a man's children with his sister's children. The effect of the four-class system is to bar the marriage of brothers with sisters and of parents with children in every case, but not the marriage of a man's children with his sister's children. The effect of the eight-class system is to bar the marriage of brothers with sisters, of parents with children, and of a man's children with his sister's children. The result of each successive dichotomy is thus to strike out another class of relations from the list of persons with whom marriage may be contracted: it is to add one more to the list of prohibited degrees" i., 279). In all these divisions the classes are indicated by totems, so that we prolong the prohibition that Crow may not marry Crow by saying that Crow (Cockatoo) may neither marry any other kind of Crow nor may he marry Eagle-Hawk (Owl) and so on.

It may be added that further regulations as to marriage may be found in certain cases outside the ordinary totem rules. A curious example of this in Melanesia will explain what is meant. In this instance, with maternal descent, whilst the marriage of a son with a mother, or of brothers and sisters, or of cousins who are the children of two brothers or of two sisters is excluded, the marriage of a father with a daughter or of cousins

who are the children of a brother and a sister respectively is not excluded. Yet neither of these unions is permitted by custom working outside the class-system; the former union is punished by death, and, as regards the latter, such cousins may not shake hands with one another or give presents to one another or even mention one another's names, and they may only converse with one another at a distance of some paces (ii., 130). It is possible that a similar idea explains the avoidance of the mother-in-law so common amongst primitive races, and of which an extreme case is mentioned from the Bank's Islands, where a man will not even walk along the beach after his mother-in-law until the rising tide has washed away the footprints which she had left behind her (ii., 76). It would seem as if, though the class-system permitted such a marriage, a better feeling rose up to declare it impossible.

It has already been mentioned that there are cases, as amongst the Arunta, where marriage with a person of the same totem is not forbidden, but this exception must be dealt with later on. The other is the general rule, namely, the custom that persons of the same totem may not marry together.

We have now to turn to the consideration of various general questions as to the origin of these strange customs, and here, needless to say, we embark on the sea of theory, leaving behind us the dry land of fact which we have hitherto been treading.

What gave rise to the idea of totems? That is

the first question with which we have to grapple, and it is not surprising that more than one hypothesis has been put forward to account for the origin of the custom. Professor Frazer, during the years in which he has ruminated over these matters, has himself put forward and abandoned two theories and now stands by a third, of which more in a moment. Let us briefly recapitulate these views.

(*i*) The theory of Herbert Spencer and, with some modifications, of Lord Avebury, that totemism originated in a misinterpretation of nicknames. Professor Frazer rules this out of court because these theories " attribute to verbal misunderstandings far more influence than verbal misunderstandings ever seem to have exercised " (iv., 44).

(*ii*) The Dutch writer, G. A. Wilken, held that totemism was explained by the belief in the transmigration of souls, but according to Professor Frazer, whose reasoning here, based on the supposed primitive character of the Australians, does not by any means satisfy us, " metempsychosis is a later product of social evolution than totemism of which indeed it may sometimes be an effect rather than the cause " (iv., 47).

(*iii*) The view of many American anthropologists, that totems were derived from the personal guardian spirits of individuals. Thus the founder of a clan acquired his totem, after the American method, by a vision, and this totem subsequently became that of the clan which he founded. This explanation, which would be admirable if there were none but American conditions to be inquired into, does not, it must be admitted, fall into line

with the ideas prevalent amongst totemistic peoples in other parts of the world.

(*iv*) Dr. Haddon's view, that totems were originally the animals or plants on which local groups of people chiefly subsisted, and after which they were named by their neighbours. This theory seems to be based on too slender a basis of custom.

(*v*) Professor Frazer's first theory, that the totem was associated with the idea of what is known to folk-lorists as "the external soul." According to this belief, embodied in many so-called nursery tales, if a man can hide away his soul somewhere, say, in a fish in the deep sea, he cannot be killed unless that fish is killed.

(*vi*) The same writer's second theory, founded on the *intichiuma* customs of which we have already spoken, that totemism originated as a system of magic, designed to supply a community with the necessaries of life, especially with food and drink (iv., 55). As these views have been abandoned by their parent we need not delay over them, but may pass to

(*vii*) His third and present theory, based practically on the evidence of one tribe, the Aruntas of Central Australia, and underlain by the fact that these people, like some other savage races, are wholly ignorant of the facts of generation and believe that conception occurs at the moment that the female first physically recognizes the fact that she is to become a mother. Then she supposes that a spirit child has taken possession of her, and from the spot where this occurrence is supposed to have taken place she comes to a conclusion as

to what kind of spirit of the dead this may be and, as a totem of one kind only belongs to any special spot, it must be obvious that the totem of the child is thus decided by the accident of position at a given moment. This accidental acquisition of the totem explains the absence of its connexion with marriage systems.

Now, before discussing this view and its rivals, the opportunity may be made use of for giving two warnings to those whose interest in early customs leads them to read the books of explorers and writers on such things. The first of these is that nothing can be a more difficult task than to get at the exact meaning of a savage's ideas through the medium of a probably difficult and almost certainly only half-understood tongue. Can we, in the case under consideration, feel quite certain Spencer and Gillen really got at the full meaning of the Arunta customs through the interpreter whom they employed? This is a question of crucial importance, and it is brought more prominently before us by the fact that the writings of Mr. Strehlow, a Lutheran missionary working in the same district, do not seem, in some important particulars, to bear out the views of the gentlemen mentioned nor those which Professor Frazer has founded upon them. The Professor does not attach as much importance to the missionary's views as others would, because the missionary's object is to turn the heathen from their ways, about which, therefore, one might suppose that he must first know something. But the Professor has utilized, as needs must be, a great deal of missionary

evidence, and where, one may safely ask, would ethnologists and folk-lorists be to-day without the details collected by missionaries of all kind? If this warning must be borne in mind, as it undoubtedly must, so also must this further warning: that even if the language is tolerably well understood, the workings of the mind of the man who employs that language, even if he wants—as he by no means always does—those workings to be understood, are not easy of comprehension. The savage is not an expert psychologist and has never concerned himself with the distinctions which arise in our more civilized minds. An example of what is meant by this may now be given. The present writer was once very anxious to ascertain whether the savage who sets up a lump of stone in his patch of yams as a protector, regards that stone as an actual god or only as the representation of a god, and made inquiry on the subject from a very distinguished writer who had spent much time as an observer amongst the peoples in question. His first reply was that the savage was not a psychologist and did not distinguish between the two ideas above mentioned. And, finally, he could not commit himself to anything further than the statement, that probably the savage in some measure considered that the godhead was focussed in the stone. This wise and cautious attitude might well be considered by the cocksure persons who theorize at second or third-hand about the views of people who may quite possibly really believe things wholly different from those which their observers suppose them to believe. Look at

the absurd things which are said about the beliefs of Catholics! The present writer calls to mind a careful book—written with no obvious anti-Catholic bias—in which it is gravely stated (1) that every priest changes his name at his ordination and (2) that the *essential part* of holy matrimony with us is the partaking of Holy Communion at the same time by the couple about to be married. Where such ideas can be held about the customs and beliefs of people of no obscure form of religion, what boundless possibilities of error must arise in connexion with the savage tribes of, say, Central Australia?

Leaving these warnings, let us turn to the actual facts of the case. Professor Frazer founds his latest theory on the assumption that the Aruntas are a really primitive totemistic race. But are they so primitive as he supposes? Those who read the account which Professor Frazer gives of them will hardly be inclined to agree with him, and especially if they also study the pages of Mr. Andrew Lang's *Secret of the Totem.*

Mr. Lang throws in his lot with those who believe that in the name of a people lies the secret of their totem, and the present writer thinks that many persons will agree with him that Professor Frazer is wrong in attaching too little importance to the sanctity and mystery attaching to a person's name amongst primitive people. It would be quite impossible even to outline here the evidence on this head. Suffice it to say that all the important theories and ideas underlying that most weighty "child's tale" of *Tom Tit Tot* go to show the truth

of the statement that the name-theory is one based on no slender foundation of fact.

If it were quite clear that the Aruntas were in an absolutely primitive state of totemism it might be legitimate to pay great respect to their isolated form of belief in connexion with the custom. But, as we have seen, this is at least a moot point. Under these circumstances we do not think that Professor Frazer has in any way shown sufficient cause for abandoning the name-theory, or indeed his own first theory, which seems to us to possess much more justification than that which he now holds.

And then as to exogamy: how are we to account for this? Here at least we know where we are as to facts. There seems to be no kind of doubt possible that the object of the custom or system is deliberately and of set purpose to prevent the marriage of near kin, and the more complicated the custom the greater the number and range of the prohibitions. But why did this arise? Judging from the nescience of the ordinary facts of generation which seems to prevail amongst some totemistic peoples we may conclude that it was no essay in eugenics which prompted this institution. What then was it? The answer is not to be found in the books of the anthropologists. Even the theory-abounding Professor throws up the sponge here, and, in words which will bear full quotation, confesses his complete ignorance on the matter. "It is impossible," he says, "to suppose that in planning it" (*i.e.*, the system of exogamy) "these ignorant and improvident savages could have been animated by exact knowledge of its consequences or by a

far-seeing care for the future welfare of their remote descendants" (iv., 168). And he proceeds to point out that the highest races do not found their unions on any system of eugenics. Further, he continues: "What idea these primitive sages and lawgivers, if we may call them so, had in their minds when they laid down the fundamental lines of the institution we cannot say with certainty; all that we know of savages leads us to suppose that it must have been what we now call a superstition, some crude notion of natural causation which to us might seem transparently false, though to them it doubtless seemed obviously true. Yet, egregiously wrong as they were in theory, they appear to have been fundamentally right in practice. What they abhorred was really evil; what they preferred was really good. Perhaps we may call their curious system an unconscious mimicry of science. The end which it accomplished was wise, though the thoughts of men who invented it were foolish. In acting as they did, these poor savages blindly obeyed the impulse of the great evolutionary forces which, in the physical world, are constantly educing higher out of lower forms of existence, and in the moral world civilization out of savagery. If that is so, exogamy has been an instrument in the hands of that unknown power, the masked wizard of history, who, by some mysterious process, some subtle alchemy, so often transmutes in the crucible of suffering the dross of folly and evil into the fine gold of wisdom and good."

Amazing stream of words! But to what do they

all amount? If the Professor, as we feel tolerably sure he does not, means by "the masked wizard of history" the Being whom we reverence as "Factorem Coeli et Terrae, Visibilium omnium et Invisibilium" we are prepared to discuss the question with him and to admit that it may well be argued that "God fulfils Himself in many ways."

But if this is not his view, and we cannot think that it is, we must be pardoned if we say, as respectfully as possible, that all this whirl of words, full of sound but meaning nothing, might have been condensed into the simple but honest phrase, "I do not know what caused these savages to adopt the system of exogamy." In which confession of ignorance many, if not all, would feel disposed to join with him.

X: STONEHENGE AND THE STARS*

AMONGST the works of Thomas Hearne and in the volume consecrated to Langtoft's *Chronicle* there is hidden away a quaint and amusing pamphlet entitled, *A Fool's Bolt soon shot at Stonage*, which is there ascribed to an anonymous author who is believed to have actually been one John Gibbons, who was a Pursuivant at Arms (1629-1718). This entertaining discourse commences with a tale of

"a wander witt of Wiltshire, rambling to Rome to gaze at Antiquities, & there skrewing himself into the company of Antiquaries, they entreated him to illustrate unto them that famous Monument in his country called STONAGE. His Answer was that he had never seen, scarce ever heard of, it. Whereupon, they kicked him out of doors, & bad him goe home, & see STONAGE; and I wish all such Æsopical Cocks as slight these admired Stones & other our domestic Monuments (by which they might be admonished, to eschew some evil, or doe some good) & scrape for barley Cornes of vanity out of foreigne dunghills, might be handled, or rather footed, as he was."

* *Stonehenge and other British Stone Monuments, astronomically considered.* By Sir Norman Lockyer. Macmillan and Co. 1906.

Recent Excavations of Stonehenge. By William Gowland. *Archæologia*, vol. lviii, p. 37.

A Bibliography of Stonehenge and Avebury. By W. Jerome Harrison. *Wilts Arch. and Nat. Hist. Magazine*, vol. xxii.

And, truly, it may be said that whilst England possesses such relics of early races as the great circles of Stonehenge and Avebury, not to speak of many other smaller but still deeply interesting examples of the same kind of edifice, there is no lack of material for the study of home-keeping antiquarians. Not that "these admired stones" have lacked attention in the past, for Mr. Harrison's most carefully compiled Bibliography runs to no less than 158 pages, an index of the amount of ink which has been spilt in expounding various theories as to the history and origin of these two great early monuments.

There is some possibility that Stonehenge was the temple alluded to by Hecatæus of Abdera, a writer who flourished about 330 B.C., when he spoke of the

"Hyperboreans [who] inhabit an island in the ocean, under the Bear, situated opposite Celtica [Gaul] and as large as Sicily. They have [he continues] a stately grove and a renowned temple of a round form, dedicated to Apollo, and adorned with many rich gifts."

Whether this "temple of a round form" was really Stonehenge is a moot point, and we have to come down to a date some thirteen hundred years later before we meet with any mention of these stones by name or any description of them. This first account is given by Henry of Huntingdon (1084-1155) in his description of the four "wonders" of England, two of which are natural curiosities

still to be seen, namely, the Peak Cavern and Cheddar Caves. The fourth wonder is this :

" That in some parts of the country the rain is seen to gather about the tops of the hills, and forthwith to fall on the plains,"

which operation of nature one would scarcely expect to meet with in a list of marvels. The second wonder is

" at Stonehenge, where stones of extraordinary dimensions are raised as columns, and others are fixed above, like lintels of immense portals ; and no one has been able to discover by what mechanism such vast masses of stone were elevated, nor for what purpose they were designed."

Geoffrey of Monmouth (1100-1154), who was Bishop of St. Asaph, gives a picturesque and wholly untruthful narrative of the origin of the monument which was afterwards adopted by that most agreeable chronicler, Giraldus Cambrensis. According to this tale the stones were originally set up on the Curragh of Kildare in Ireland, and there known as the " Giant's Dance." Now about 470 A.D. Hengist treacherously massacred 460 British chieftains " at the monastery of Ambrius," and Aurelius Ambrosius, desiring to erect a monument to this little army of princes, consulted Merlin, and was advised by him to steal the Giant's Dance from Ireland, a suggestion which seems to lend quite a touch of verisimilitude to an otherwise unconvincing narrative. Further, Merlin told his sovereign that these stones were not originally

Irish, but had been brought to that country by giants from Africa, under which circumstances it was obvious that they were the rightful possession of England. At any rate Merlin was dispatched with an army to Kildare, and the stones were removed and set up on Salisbury Plain, near the "Mount of Ambrius," and subsequently served as the burial-place of Aurelius, of Uther Pendragon and of Constantine.

The author of the *Fool's Bolt* thought

"surely, it was some heathonish temple demolished by the immediate hand of God, as an intollerable abomination unto him: yet reserving so much of it standing, as may declare what the whole was, & how, & why, so destroyed, that, as we are to remember Lot's wife, turned into a Piller of Salt, for looking backward towards idolatrous Sodome, so we should remember, that these forlorne Pillers of Stone are left to our remembrances, dissuading us from looking back in our hearts upon anything of Idolatry, and persuading us, in imitation of Moses and the Prophets, so to describe and deride, it in it's uglie Coullers, that none of us, or our posterity, may return with Doggs to such Vomit, or Sows to wallowing in such mire."

The surmises as to the nature and origin of Stonehenge have been most numerous and most varied. Of course it has been associated with the Druids, the last resort of all in search of an author for any ancient monument, and according to one writer these Druids were a Phœnician colony who came to Britain in the time of Abraham, and

brought the patriarchal religion—whatever that may have been—with them, whilst another writer says that the Druids were certainly Brahmins and that "Stonehenge is evidently one of the temples of Boodh."

Another effort explains it as a monument erected to Boudicca, whom we used to call Boadicea in the uninstructed days of our youth, and the convincing arguments by which this claim is established are (1) that the battle in which that ill-fated queen was killed was fought upon a plain, and (2) that Dion Cassius, the historian, tells us that the Britons "intombed their Queen with solemn and magnificent pomp." These arguments are quite in the style of some of those set forward in our own day for the establishment of the theses of the wilder kind of folk-lorist.

Stonehenge has been called—with no shred of reason—a Mithraic shrine, and it has been also suggested that it might have been a sort of British "Tower of Silence," where dead bodies were laid to be devoured by birds and insects, wild beasts being kept off by a kind of zareba of thorns inserted between the upright pillars of the trilithons.

Stukeley, an imaginative archæologist who assigned reasons and names for ancient objects because it struck him that such reasons or names were pretty and attractive, thought that Stonehenge and other like edifices were consecrated to snake-worship. He gave the name of Dracontium to such an edifice and has a pretty but largely imaginative rendering of Avebury—that greater Stonehenge in Northern Wilts—as a snake, with

an eye, passing through a circle and weaves quite a thread of folk-lore around his mythical design.

The sagacious and experienced reader will scarcely fail to expect that, in the midst of so much misdirected ingenuity, there will be found some attempt to connect Stonehenge with the "number of the beast."

Oddly enough, I can find no such effort, though it can hardly be supposed that it has not been made. Certainly, within the last few years one of the Scotch Stone Circles has been so associated, and the writer of the paper in question goes so far as to say that "it is obvious that had it not been for the 'number of the beast, six hundred threescore and six,' in the Apocalypse," the distance and numbers which he has worked out in connexion with this circle "would have been without meaning." And he asks, "Cannot we go back, in imagination, 1,850 years, to the island of Patmos, and see a converted Phœnician High Priest laying at the feet of the beloved Disciple his once most cherished possession, the Secret Number of the Sun God?"

At the end of this list of explanations—and it might have been made longer—the reader will perhaps find himself inclined to exclaim with Pepys, "God knows what their use was! they are hard to tell, but yet may be told."

It now remains to be seen whether recent researches have thrown any light upon the date and purpose of stone circles in general and of Stonehenge in particular.

On the last day of the last century two of the

stones of the outer circle and the lintel thereof fell to the ground, and this fact coupled with the obviously insecure condition of some of the other stones and the recent formation of a great military camp in the immediate vicinity of the monument led to steps being taken to secure Stonehenge from further damage and to make good that which had recently occurred. The work was carried out under the immediate direction of Professor Gowland, than whom no more competent person could possibly have been found, and the results which have been obtained from the necessary excavations have certainly thrown much light upon the period when these stones were set up. Before mentioning what these are, it may be well, for the sake of those unfamiliar with the subject, to call attention to two points peculiar to this monument. In the first place, then, whilst it resembles in certain respects other and smaller circles in different parts of the island, it differs from all of them in the fact that its stones have been shaped whilst all the others are constructed with rough, undressed pillars. Moreover, the lintels which surmount the trilithons, which may roughly be described as a species of stone doorways, are not merely laid on the tops of the stones which support them, but are fitted thereto by means of a kind of mortice and tenon joint, for on the top of each upright there is a stone peg which fits or fitted into a corresponding recess on the under surface of the lintel. This superior workmanship has always been held to have been a proof that Stonehenge was later in erection than the other monuments of a

similar kind. Even the much more magnificent ruin of Avebury, which in Charles II's time was said by Aubrey to "as much exceed Stonehenge as a cathedral doth a parish church," was made of rough, unhewn stones and so has been looked upon as an earlier edifice than its southern sister.

Then, in the next place, the stones at Stonehenge are of two kinds. The greater number and the larger stones are the so-called "sarsens" of the district, sandstone blocks which in certain places, for example at Clatford Bottom, between Marlborough and Avebury, may be seen as regular rivers of stone. No doubt in early days, before many of them had been used up for road-mending, boundary stones and the like purposes, there must have been many more of these great blocks in existence over the surface of Salisbury Plain, so that the builders of the temple had, in all probability, no very great distance to go in search of the materials for their edifice. But in addition to these great stones there is an interior horseshoe-shaped arrangement of stones of a different character. These "blue stones," as they have long been called, are of a different nature from the others, being mostly what is known as porphyritic diabase, and, as there are no stones of this kind on the plain, nor, indeed, within many miles of Stonehenge, it was thought that they must have been carried from a distance and that they were perhaps the sacred stones of some distant tribe who had brought them to the Plain when migrating there themselves; had set them up as sacred objects; and, finally, had surrounded them with

the great trilithons of local stone which complete the edifice.

All this, it appears, may now be regarded as exploded, or at the least as most doubtful, for Professor Judd thinks that there is no reason to suppose that these " blue stones " might not have been found on the Plain with the sarsens. They must have been transported there, for they are certainly foreign to the locality, but then there is no reason why they might not have been brought there as glacial drift, deposited and found ready to their hands by the builders of Stonehenge.

Turning next to the dressing of the stones, the observations of Professor Gowland seem to make it clear that this was accomplished almost entirely by the use of stone implements. A certain amount of the rough shaping may have been effected by the use of fire and water, as we know has been the case with monuments erected in our own times by primitive races. But the final tooling seems to have been carried out by means of flint axes and large stone mauls made of the compact sarsen stones met with on the Plain. Large quantities of these implements were found in the course of the excavations, indeed used-up axes and mauls had been employed to pack the bases of the upright pillars in the holes dug for their reception. Moreover, it was observed that the faces of all the stones showed evidence of very careful tooling, and this more especially where they had been protected from the weather. That this tooling was effected by means of quartzite hammers seems to have been demonstrated by the fact that the

foreman mason was able to produce with these hammers an exactly similar kind of tooling on a piece of sarsen whereas he was quite unable to produce a similar appearance on the same stone by the use of any of his own mason's tools. There can be little doubt that Professor Gowland is perfectly correct in assigning the rough hammers and mauls which he discovered to the neolithic or later stone period. Their roughness of construction is no argument to the contrary, for it is quite clear that early man had sense enough not to make a razor for the purpose of cutting a granite block, and that he reserved his highly finished and polished implements for better purposes than that of tooling great blocks of sarsen, a task which could be perfectly well carried out by less carefully modelled and finished implements.

There is one further point connected with the period of construction of Stonehenge which must not be omitted. The eagle eye of Professor Gowland detected a small patch of green incrustation upon the base of one of the tooled slabs; and a portion of this having been analysed, it was proved that the incrustation was carbonate of copper and that it could only have been produced by prolonged contact with a small lump of copper or bronze, or other alloy of copper or with some small ornament made of a substance of this nature. A prolonged search was made for any remains of this object, but without success, though Professor Gowland is quite sure that nothing which was larger than one-eighth inch could possibly have

escaped his scrutiny. We may suppose, therefore, that all the object must have been converted into carbonate; but, at any rate, it is quite clear that there must have been something of a coppery nature in contact with the stone and, therefore, that it must have been erected at a period when that metal was known. That the tooling was done with stone implements is no bar to the possibility of Stonehenge having been erected during what is known as the Bronze period. Many of the most shapely and most carefully executed stone implements seem undoubtedly to have been made during that period and even during the Iron Age in England, and one must not forget that many of the combatants even at Senlac fought with stone mauls. At the same time it seems probable, to say the least, that if Stonehenge had been erected during the full swing of the Bronze Age some implements of that material would have been found during the progress of the excavation, which, however, was not the case. From the fact, then, that copper was known, but that no metal implements were discovered, we may draw the conclusion that the monument was erected at the very beginning of what is known as the Bronze Age, and we may set ourselves to inquire when that Age may be supposed to have commenced in Britain. Here we enter a region where surmise alone is possible, but it may be said that Montelius, who is a leading authority on the subject, has assigned the date of about 2000 B.C. as the probable period of the commencement of this Age in Northern Italy. Sir John Evans suggests

1400 B.C. as the appropriate date for Britain, but thinks that his estimate probably errs by being too near our own times. Professor Gowland agrees with this latter view, and thinks that a country where copper and tin were both so accessible and so easily discoverable—and "no country in the world presented greater facilities for their discovery"—would also be a country in which they would come comparatively early into use. He thinks that it would be safe to date the commencement of the Bronze Age in Britain as far back as 1800 B.C. and to assign to a similar date the construction of Stonehenge. This is, perhaps, as near an approximation to the date of this famous monument as we can ever hope to reach.

It now remains to be seen whether any further light has been thrown upon the origin or use of this edifice. There seems little doubt that Stonehenge and its kindred must have been religious temples of some kind. It is difficult to suppose that any other incentive than one of a religious or a military character would have led to the construction of buildings and earthworks which must have cost such a vast amount of time and labour. Military they clearly were not, for even at Avebury, where there is a gigantic fosse and vallum enclosing some twenty-eight acres, these structures are turned in the reverse way to those of military earthworks, and, indeed, are constructed in such a manner as to be a source of danger rather than of protection to those within their boundary, should the place be attacked. By a process of elimination we arrive, then, at a religious origin,

and this view is strengthened by the vast number of burials which exist in the immediate vicinity of Stonehenge. Indeed, this part of the Plain may almost be considered to resemble the graveyard which surrounds a country church.

But if the object was religious, can we go any further and say what was the object of worship? Sir Arthur Evans, whose opinion on any matter of this kind is worthy of the most careful attention, thinks that the central object of worship in Stonehenge was an oak-tree, "the Celtic image of Zeus" according to Maximus Tyrius. He also is of opinion that the whole edifice is really a kind of enlarged model of the sepulchres of the dead, and is associated, therefore, with the idea of a future life and perhaps with the worship of deceased chieftains or relatives. The outer circle of stones is the descendant of the hedge of stones which surrounded the barrow, or was placed just within its outer edge; the avenue of stones which is imperfect at Stonehenge, but well marked at Avebury and at some other places, represents the underground gallery which in the case of some long barrows, such as those at Uley and West Kennett, leads into the place of sepulture; whilst the central dolmen, which is wanting at Stonehenge but is present in some other instances, is an actual or perhaps a ritual place of interment.

Others, and these not all of the present day, have associated Stonehenge and other circles with the worship of the heavenly bodies. One ingenious writer surmised that the Druids had laid out a kind of celestial map on the Wiltshire Downs, in

which five planets were shown. Stonehenge represented the planet Saturn: the two circles at Avebury stood for the Sun and Moon, and they all centred round that most marvellous and wholly unexplained earthwork, Silbury Hill, which was taken to mean the Earth. Dr. John Smith, who published in 1770 a work called *Choir-Gaur*, dealing with Stonehenge, which he calls "the Grand Orrery of the Ancient Druids," gets nearer to modern theories when he describes it as an astronomical "Temple erected in the earliest ages for observing the motions of the heavenly bodies." Further, he goes on to say that the Stone, at some little distance from the circle itself, which is known as the "Friar's Heel," indicates the "sun's greatest amplitude at the summer solstice." This large stone is a point of importance in all solar and stellar theories in connexion with Stonehenge, and the guide who used, in my earlier days, to show people over the place, always used to point it out with the remark that the sun rose over the Friar's Heel on Midsummer morning, and shone straight over the Altar Stone, and between the pillars of the great central trilithon. As a matter of fact this statement is correct, for Professor Gowland says that

"If on the morning of Midsummer Day we stand in the middle of the horseshoe curve in which the trilithons are arranged, a point once marked by the aperture between the two piers of the central and greatest of them, and look in the direction of the "hele stone" [*i.e.*, the Friar's

Heel], the sun will be seen to rise approximately over the summit of that monolith."

And he proceeds :

"This can hardly be accidental. It is, in fact, impossible to conceive that the arrangement of the trilithons in an open curve, with its opening directed eastwards, and more especially that the position of the central trilithon and altar stone in relation to the 'hele' stone and the avenue, can have been the result of mere chance. If not the result of chance, the disposition of the stones must have been made with some purpose, and that purpose cannot have been other than to direct observers or worshippers to the point where the sun rose in the heavens."

And he strengthens his argument by producing similar instances from Japan, where sun-worship is actually practised. It is obvious that Stonehenge might have been oriented in connexion with a star, and in fact it was suggested in the *Astronomical Register* that Sirius was the star in question, and that in 977 B.C. it rose exactly above the Friar's Heel, a fact which the writer supposed would fix the date of that monument.

Moreover, Piazzi Smith and Sir Norman Lockyer have both of them endeavoured to show that the Pyramids were oriented in respect of certain stars, and Mr. Penrose has advanced similar theories with regard to the orientation of some of the Greek temples.

However, the sun in the case of Stonehenge seems a more likely object than any star, having

regard to what has been said as to its rising at the summer solstice; and if Sir Norman Lockyer's view that it is a solar temple is correct, we may also perhaps accept his further statement that astronomical data point to its having been erected somewhere about 1700 years B.C., a date which very closely approximates to that arrived at by Professor Gowland by quite a different line of argument.

Sir Norman thinks that Stonehenge and many humbler edifices, such as dolmens and the like, were really astronomical temples erected by the priests of the period, and that their purpose was to indicate the seasons with a view to agricultural operations. As he says, nowadays anybody can go into a shop and buy an almanack for one penny or, one may add, may even obtain it gratis from his grocer or some other tradesman who desires to keep himself before the mind of a possible customer. Hence, there is no reason why the generality of mankind should study astronomy or desire to know how they may learn from the heavens what time of the year it may happen to be. But in the days before calendars it was of the utmost importance that the farmer should know when he was to plough and when to sow, and this information, it is urged, he obtained from his priests, who were his priests just because they could give him the information in question.

Of course we all know that our date for commencing our year is purely arbitrary, that the year did not always begin on January 1st and that it might be made to begin on almost any day. And,

may have had some symbolical and esoteric meaning to those who erected the monument, quite apart from any utilitarian motive connected with agricultural operations. Nor, certainly, is there any proof that whatever kind of priesthood may have existed amongst these shadowy predecessors of other races in England owed its existence to the knowledge which its members possessed of the seasons and their power, and consequently, of telling when certain agricultural labours should be undertaken. Nor, again, in our opinion, is there any kind of evidence that many, perhaps any, of the dolmens were ever star or sun observatories for similar purposes, since their orientation seems to us to have depended far more probably upon some religious motive than on any other.

Those who have paid any attention to folk-lore —and these considerations may be judged to belong in some measure to the domain of that fascinating branch of study—will not have failed to note that there are dominant fashions there as in other and more mundane affairs.

Just now the solar theory of monuments is such a fashion, but it may find itself replaced some day by another, as it has supplanted the snake theory and the horticultural theories of other writers.

At the same time the evidence in connexion with Stonehenge and with some other circles is so strong that it will require a great deal of proof to the contrary to show that they were not connected in some way with the worship of the sun, whatever may have been the special significance of the peculiar method of orientation followed in each.

XI: WHO WERE THE FAIRIES?

IN his *Demonology and Witchcraft*, a now almost forgotten work of that far. from forgotten author Sir Walter Scott, allusion is made to the strange history of a certain Robert Kirk, M.A., Minister of Aberfoyle. (in Scotland), in 1691. Kirk was a minister, the son of a minister and the father of a minister, in which facts he resembled many of his Presbyterian brethren in former and present days. For our present purpose it is much more important to remember that he was a seventh son and thus endowed, according to common legend, with strange psychic powers and liable to strange experiences not given to other men.

Such, if his story be true, was certainly the fate of the Rev. Robert Kirk, for although he is stated to have died in 1692 and although Scott saw his tombstone inscribed

† ROBERTUS KIRK, A.M.
Linguæ Hiberniæ Lumen.

in spite of these facts, according to common report, Kirk never did die but was carried off into

* *The Secret Commonwealth of Elves, Fauns and Fairies.* Ed. A. Lang. Nutt. 1893.
Tyson's Pigmies of the Ancients. Ed. B. C. A. Windle. Nutt. 1894.
The Testimony of Tradition, MacRitchie. Kegan Paul. 1890.
The Fairie Faith in Celtic Countries. Wentz. Frowde. 1911.

† The laudatory sentence relates to his having made a translation of the Bible into Irish.

Fairyland by its outraged inhabitants for reasons which will shortly appear.

It is true that he appeared to die, that is, he sank in a swoon on the side of a *dun-sidh* or fairy hill, and that a funeral was held in which he was the silent participant, but, according to the legend, all this was a mere pretence. "After the ceremony of a seeming funeral" says Scott, "the form of the Rev. Robert Kirk appeared to a relation, and commanded him to go to Grahame of Duchray: 'Say to Duchray, who is my cousin as well as your own, that I am not dead, but a captive in Fairyland; and only one chance remains for my liberation. When the posthumous child, of which my wife has been delivered since my disappearance, shall be brought to baptism, I will appear in the room, when, if Duchray shall throw over my head the knife or dirk which he holds in his hand, I may be restored to society; but if this be neglected, I am lost for ever." We note in passing the introduction of the sacred metal, iron, as a talisman against fairy devices and await the sequel. Kirk appeared at the baptism and was seen by Duchray and, as it would appear, by others then present, but whether Duchray had no desire for the return to earth of his cousin, or whether he was, as Andrew Lang believed, so much astonished as to forget the message, the fact remains that the dirk was not thrown and Mr. Kirk once more returned to his fairy prison where we must assume he still languishes. And now we may ask why his gaolers were so angry with Mr. Kirk. It was presumably because he knew too much about

them and had made the world his confidant by publishing the work whose name appears first in the list at the head of this chapter. *The Secret Commonwealth* was an exceedingly rare book, and indeed may still so be described since the late Mr. Nutt's reprint of it in his *Bibliothèque de Carabas* with a learned introduction by the late Andrew Lang was very strictly limited in numbers.

It is a book well worth reading, and is distinguished from every other book on the subject by the fact that its author treated his subject in exactly the same manner as he might have done had he been writing an account of the manners and customs of the inhabitants of the village of Aberfoyle over which he was minister. Galt's *Annals of a Parish* is the kind of book of which I am thinking, but that was admittedly a work of imagination. Not so Mr. Kirk's essay: that was in his hands as much a sober account of actual things as was his countryman Mungo Parke's description of his journeyings in Africa, and of the tribes and peoples with whom he met. The *Secret Commonwealth*, then, is of great value, because it sets down once for all exactly what things were believed about the Fairies, in Scotland at any rate, at a time when such beings were as much believed in as were witches and warlocks and other like uncanny and, usually, unseen, existences. For the full account the curious must be referred to the pages of Mr. Lang's edition, and those who betake themselves to it will find a rich reward, but a few of the more general passages must be quoted in order that Kirk's position with regard

to these beings may become clear. The Fairies he says, "are said to be of a middle nature betwixt Man and Angels, as were Daemons thought to be of old; of intelligent studious spirits, and light changeable bodies (lyke those called Astral), somewhat of the Nature of a condensed Cloud, and best seen in Twilight. Thes Bodies be so plyable through the Subtilty of the Spirits that agitate them, that they can make them appear or disappear att Pleasure. Some have Bodies or Vehicles so spungious, thin and defecat, that they are fed by only sucking into some fine spirituous Liquors, that peirce lyke pure Air and Oyl: others feid more gross on the Foyson or substance of Corns and Liquors, or Corne it selfe that grows on the surface of the earth, which these Fairies, steall away, partly invisible, partly preying on the Grain as do Crowes and Mice; wherefore in this same Age, they are sometimes heard to bake Bread, strike Hammers, and do such lyke Services within the little Hillocks they most haunt: some whereof of old, before the Gospel dispelled Paganism, and in some Barbarous places as yet, enter houses after all are at rest, and set the Kitchens in order, cleaning all the Vessels. Such Drags goe under the name of Brownies." And "Such Drags," we may feel certain many a mistress would be glad to lure to her household to-day. Not to quote further, Kirk draws for us a picture of a body of people usually but not always invisible; when seen, most generally by night and not by day; inhabiting hillocks and mounds of an artificial character (many raths in Ireland were and to some extent still are associated

with them); stealing cattle, children, goods from their human neighbours to whom they sometimes act in the capacity of midwives; wandering from place to place; sometimes kindly, sometimes revengeful—in a word the fairies with whom we learnt to be familiar when children from our story books.

Who were these fairies? From what did all these tales flow? Were they pure efforts of imagination or is there an underlying stratum of actual fact beneath these seeming trifles? It is the problem of Euhemerus once more, and many are the efforts which have been made to solve it, not, it must so far be admitted, with any very conspicuous success. Some there are like Kirk, who dispose readily of the matter by asserting that there really are fairies, a solution which would of course at once account for the belief in their existence. Of these is Mr. Wentz who came over from America, where there are plenty of legends of little people though no true fairies, to find out what he could find out about the genuine article. The result is the truly amazing book which must be treated with respect, since it has won for its writer degrees in Academies no less distinguished than the Universities of Rennes and Oxford (to place them in the chronological order in which the degrees were obtained). It is not a wholly satisfactory guide even to things as to which our knowledge is sufficient, as may be instanced by the assertion that the Catholic Doctrine of Purgatory was derived from the Celtic notion of Re-Birth, and might be made evident by many another example,

if this were a review of the book in question. But over and above all this, Mr. Wentz's book is of interest because he took the trouble to visit the chief centres of fairy-lore and to interrogate those from whom he believed that he might get information. The unkind might remark that he got what he went out go get and what he might have expected to get—but let that pass. He returned from his inquiries with a firm faith in the fairies and, if we are to credit our eyesight, also with a sincere desire, shared, so he states, by some at least in Ireland, for "the relighting of the Fires and the restoration of the old Druidic Mysteries," as to which it may parenthetically be added, we know very little, yet from what very little we do know can scarcely imagine any persons desirous of their restoration. However, without accepting Mr. Wentz's easy way, we may at least, admit that he has been successful in once more awakening an interest in the problem stated at the head of this article.

Now, in approaching all questions of myth like that of the Fairies or of the Tuatha de Danaan, let us say, hastening to add that we not for a moment indicate that the two cycles of tales have any relationship with one another, there are two ways of looking at the matter.

We may take the view that the whole thing is *ab initio* a myth and arose in the minds of men without any substantial foundation in fact, or we may take the view that there is an underlying fact or an underlying man or woman obscured almost or quite from recognition by the cairn of accretions

erected over his name. This was the view of Euhemerus in the fourth century, B.C.; it has been the view of many since; it is not too much perhaps to say that it is the view of many, perhaps most, of those most competent to form an opinion to-day. Such a view would teach that behind the legendary Arthur of Malory and Tennyson there was a real Artorius or some such named man around whom many extraneous legends accumulated. Such men recognize the amazing toughness of tradition, and it is certain that they can support their belief in its toughness by many remarkable examples. At any rate, such an attitude underlies Mr. MacRitchie's well-known effort to explain the fairies.

According to his view the stories about the fairies are really stories, half-forgotten, almost wholly perverted, entirely misunderstood, respecting early races of this and other countries, but especially these islands: tales told of them by an aftercoming invading race which may often have had to suffer from a pin-prick policy on the part of those whom it had dispossessed. And he points out that such a race of dispossessed inhabitants would naturally, when not actually enslaved, lead a furtive, underground, nocturnal existence as the fairies are fabled to do. They might from time to time have been called in to act as midwives, perhaps from that belief in their magic which all conquering races seem to have accorded to those whom they had conquered; they very likely did on occasion steal from their supplanters not merely food and other articles, but sometimes children. In

fact it is not difficult at all to explain in some at least of the more important fairy characteristics of the legends in terms of this theory.

Let us consider a couple of examples. Most people have heard of what are called "elf-darts," *i.e.*, pre-historic stone arrow heads: they are to be seen in almost every museum. Kirk says of them: "Their Weapons are most what solid earthly Bodies, nothing of Iron, but much of Stone, like to yellow soft Flint Spa, shaped like a barbed Arrow-head, but flung like a Dairt, with great Force. These Armes (cut by Airt and Tools it seems beyond humane) have something of the Nature of Thunderbolt subtilty, and mortally wounding the vital Parts without breaking the Skin; of which Wounds I have observed in Beasts, and felt them with my Hands." And that we may take some comfort against such dexterous and invisible opponents, he adds: "They are not as infallible Benjamites, hitting at a Hair's-breadth; nor are they wholly unvanquishable, at least in appearance." In stating this, Kirk is simply stating one of the best-known bits of folk-lore that we have. There is, to take but one of many other examples, another amusing account of the same idea in a letter written in 1700 by Dr. Hicks to Pepys, in which, after a somewhat similar description to that given by Kirk, he concludes: "I have another strange story, but very well attested, of an Elf arrow that was shot at a venerable Irish Bishop" (no doubt, from the date, a Bishop of the Establishment) "by an Evil Spirit, in a terrible noise louder than thunder, which shaked the

house where the Bishop was." Now, as far as the piece of folk-lore to which we have just been devoting our attention goes, there can be nothing more certain than that the "elf-darts" are the stone arrow-heads of early races which inhabited these islands and parts of Europe before history was and in so far there certainly seems to be a nexus between the tale and the fact.

Take again a tale like that of Child Rowland, so excellently told by Mr. J. Jacobs and illustrated by Mr. H. J. Ford in the first *English Fairy Tales*. In the notes to this tale Mr. Jacobs alludes to its having been known to Shakespere who mentions it in King Lear :

Childe Rowland to the Dark Tower came

and believes that Milton used its original form as the substructure of the poem which he wove around the loss of Sidney's daughter in the woods near Ludlow—the Masque of Comus.

Over these literary connexions we must not linger, but in a brief manner must indicate the outlines of the story. A girl is playing ball with her three brothers. In the course of the game she commits the indiscretion of running round the church "withershins," *i.e.*, in the opposite way to the sun which, as all know, is the very way to deliver oneself into the power of the fairies. That is just what happens in her case, for the King of Elf-land carries her off to his tower from which she is only to be brought back by the boldest knight in Christendom. The two elder brothers assay the task, but neither of them returning, it

falls to the lot of Childe Rowland, the youngest, to make the adventure and deliver his sister. Directed by various people he reaches the Dark Tower, and note how it is described. It is "a round green hill, surrounded with terrace-rings, from the bottom to the top." Thus it resembles one of those mounds or "earth-houses" which form a part of Mr. MacRitchie's argument. In the interior it is gorgeous beyond all possibility of belief, but it may be admitted that it was pardonable for the teller to let himself go a little at this point. At any rate, there were Burd Helen and the two unsuccessful brothers, and the youngest would have joined them in their confinement had he not remembered the directions which he had received to fast from all food while in the Tower. There follows a conflict with the King of Elf-land who is defeated, and has to render up his prisoners.

Let us first strip this tale of three well-known bits of folk-lore, only the last of which has any relation to our present subject.

There is the "Withershins" matter, a commonplace of folk-lore, like the "success of the youngest brother," another ordinary incident. And finally there is the "abstinence from food whilst in Fairyland," of which more presently. Stripped of these and of the obvious efforts of the imagination respecting the size and adornments of the Dark Tower, we certainly have a series of events which might easily be translated into the carrying off of a daughter of the conquering race by one of the conquered; of her imprisonment in some hidden place of security; of her rescue by her relatives.

In a word, the two stories have convertible terms.

The matter is, however, not quite so simple as might appear from what has just been said. Mr. MacRitchie's thesis contains the suggestion that the early tribes whose recollections have become handed down to later generations as fairy tales were small-sized, even pigmy people.

Now it is undoubtedly true that there are pigmy races in certain parts of the world. There is the well-known case of the pigmies of the Central African forests discovered by Stanley, and in this connexion one may note that pigmy races in Africa were mentioned by Aristotle. "They (the Cranes) came out of Scythia to the Lakes above Egypt whence the Nile flows! This is the place whereabouts the Pigmies dwell. For this is no fable but a truth. Both they and the horses, as 'tis said, are of a small kind. They are Troglodytes and live in caves." And the same tale appears in Homer:

So when the inclement winters vex the plain
With piercing frosts or thick-descending rain,
To warmer seas the cranes embodied fly,
With noise and order, through the midway sky;
To pigmy nations wounds and death they bring
And all the war descends upon the wing.

For a long time it was supposed that these stories were but stories and had no foundation of fact, and Tyson's Essay, which was a sort of appendix to his *Anatomy of a Pigmy, i.e.*, of a Chimpanzee —one of the first attempts at a study in Comparative Anatomy—was intended to show that the

pigmies of the ancients were really monkeys and thus was one of the earliest essays in euhemeristic folk-lore of what may be called modern times. *

Further, there are legends, many of a very ancient character, of small races in various parts of the world, such as China and Japan and North America. But the important fact is that in the places where what may legitimately be called the fairy legend most flourishes there is no evidence that there ever was at any period of history anything in the nature of a pigmy race.

This may fairly be said to be the case all over Europe, and it is most emphatically true of these islands. Whatever may have been the differences between the appearances of the earlier races—whom we only know by their skeletons—and the race as we now know it, and the skeletal differences are surprisingly small, there is no great and remarkable difference of stature between them. There are stunted races like the Lapps and the Esquimaux, but these, strictly speaking, are not pigmy races, nor is there any evidence that they or any like them inhabited these islands at any time or were sufficiently well-known there to form the substratum of a legend. It is now nearly twenty years since I published my book on the Pigmies, and the statement just made was one which appeared in essence in that work. It has only been confirmed by all recent evidence, and much has come to light during the period of time alluded

* Edward Tyson, M.D., F.R.S., published his book in 1699, His "pigmy" is, I believe—or at least its skeleton is—in the Natural History Museum at South Kensington.

to. Certainly nothing can be better proved than that the flint arrow-heads to which allusion has been made were not constructed by a pigmy race, and as to the so-called "pigmy implements" these owe their name to their size. There is no sort of osteological evidence that their makers were pigmies themselves.

Further, there is the remarkable and highly significant fact that many of the mounds associated with fairy legends were never habitations of the living, but they were the resting places of the dead. Such was the well-known Bryn-yr-Ellyllon or Fairy Hill, near Mold, and such the sepulchral barrow of Willey How in Yorkshire. It is partly on this fact that the late Mr. Andrew Lang founded his belief that the real origin of the belief in fairies was to be found in "a lingering memory of the Chthonian beings, 'the Ancestors.'" He emphasizes, in fact, the importance of "the part played by ancestral spirits, naturally earth-dwellers." And, in this connexion, appears the significance of one of the pieces of folk-lore embedded in the tale of Childe Rowland, that of the abstinence from food. For as he points out "in many ways, as when persons carried off to Fairyland meet relations or friends lately deceased, who warn them, as Persephone and Steenie Steenson were warned"—and, we may add, as Childe Rowland was warned—"to eat no food in this place, Fairyland is clearly a memory of the pre-Christian Hades." And he continues: "There are other elements in the complex mass of Fairy tradition, but Chaucer knew 'the Fairy Queen Proserpina'

as Campion calls her, and it is plain that in very fact the dread Persephone,' the 'Queen over death and the dead,' had dwindled into the lady who borrows Tamlane in the ballad. Indeed, Kirk mentions but does not approve of this explanation 'that those subterranean people are departed souls.' Now . . . the dead are dwellers under earth."

But, even if we grant that Mr. Lang was right in his hypothesis, and there is far more evidence in favour of it than it has been possible to set down here, we are no more precluded than he was from agreeing that in Mr. MacRitchie's theory there is a powerful adjuvant in the building up of the fairy mythology. Given an idea of a people living under ground—the souls of the dead—we naturally arrive at the idea of a small people for many primitive races seem to have pictured the soul as a small replica of the man. Thus the Macusi Indians believe that although the body will decay "the man in our eyes" will not die but wander about. Given this fundamental idea it is quite easy to see how accretions, derived from wholly different sources, may have become added to it. If there is one thing obvious about mythology it is that tales get told about persons to whom they never belonged until they become part of the stock history or legend.

And in the same way it is quite easy to see how the doings of a conquered but unenslaved race, hiding from their conquerors by day and only venturing forth at night on stealthy forays; intermarrying occasionally, as the fairies are said to

have done with men; comporting themselves, in a word, after the manner of non-magical fairies, may have got mixed up with the earlier tradition until they became inseparably blended with it. There are probably other strands in the tangled skein of fairy legend: perhaps those indicated in these pages may be two of them.

Before concluding it may not be amiss to say a few words on an exceedingly interesting point to which that very distinguished scholar Professor R. A. S. Macalister has recently called attention.

If there is one part of the world which is "fulfilled of faery," it is Ireland. Sir William Wilde said that every green rath (*i.e.*, earthen fortress) was consecrated to the good people. There I think he spoke too largely, for the people now at any rate distinguish between "faery-raths" and what are sometimes at least called "Danish-raths," though I have never been able to find out on what the distinction is based. Still the fact remains that an immense number of these raths were and even still are, though the belief seems to be dying out, associated with fairy legends. To some these fairies are the genuine descendants of the Sidhe or Aes Sidhe, *i.e.*, "the hill-dwellers," an ancient Irish name for the gods. Those who wish to know something about them and something about Celtic Mythology in general may be advised to spend one penny on Professor McNeile's most admirable pamphlet.* Space will not permit of this matter being touched upon here, but the interesting

* Published by the Catholic Truth Society in their series on the History of Religions.

point which Professor Macalister wishes to emphasize is that "there is a complete break in continuity between the supernatural beings of the old Irish romances and those spoken of by the modern peasant. The modern fairies in manners, customs, garb, appearance, and names are as different as possible from the Aes Sidhe and the other beings of whom tales are told in the MSS." And he takes an example which may be dwelt upon for a moment. All over Ireland we find the Puca or Pook embedded in place-names. There is for example the Waterfall of Poul-a-Phuca near Blessington: there is a Carrig-a-Phuca, that curious peel-tower on the top of a rock which cannot fail to catch the eye of any tourist motoring from Macroom to Killarney, there is Castle-Pook close to Doneraile, where Canon Sheehan lived and is buried: there are a host of other examples. The name itself ought to arouse our suspicions as to a foreign origin for its bearer, for what has a fairy with an initial P to do with a country of K-Celts? And as a fact Professor Macalister declares that as far as the Puca is concerned "the delvers in ancient Irish literature have not yet unearthed the faintest trace of such a being or anything like him." From these two facts it would seem as if the Puca must be an alien importation, and such Professor Macalister believes him to have been. He thinks that the fairy as now and in recent times known in Ireland is much more like the Shakesperian sprite than he is to the more ancient being of legend. No one will need to be reminded that Shakespeare knew about Puck.

In the past it was thought by some that the poet had appropriated him from some Irish acquaintance as he must have done those six or seven Irish songs with whose orthography he played such havoc; a puzzle to all the commentators until the difficulty was cleared up. Now it would appear that the current set in the opposite direction, and that the fairy of recent years was a bye-product of the Anglo-Norman invasion of Ireland. Such is the theory now propounded and Professor Macalister explains by it the fact that fairy-legends are attached to ring-forts or raths. "For," he says, "these ancient steadings are not as a rule ancient. They frequently have Ogham stones used up as a building material in their construction, and we must allow time for a number of stages of historical development before the forts could be ready for their supernatural occupants. First we must assume the representatives of the owner of the monument" (*i.e.*, the Ogham stone, these being inscribed tombstones) "to have become extinct, before the stone could be appropriated. Then we must allow time for the family of the occupant of the fort to become extinct. Then we must allow further time for the fort to lie derelict and finally to fall into ruin; and we need then a further space for all recollection that it and places like it were once centres of human habitation, to be effaced from local popular memory. If we start with a monument of the sixth or seventh century, we will arrive at a date not very far off the Anglo-Norman conquest by the time all this has happened."

It is an exceedingly interesting theory, and one which we may hope that the writer from whom we have quoted will yet more completely elaborate in the future.

INDEX OF NAMES

GENERAL INDEX

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